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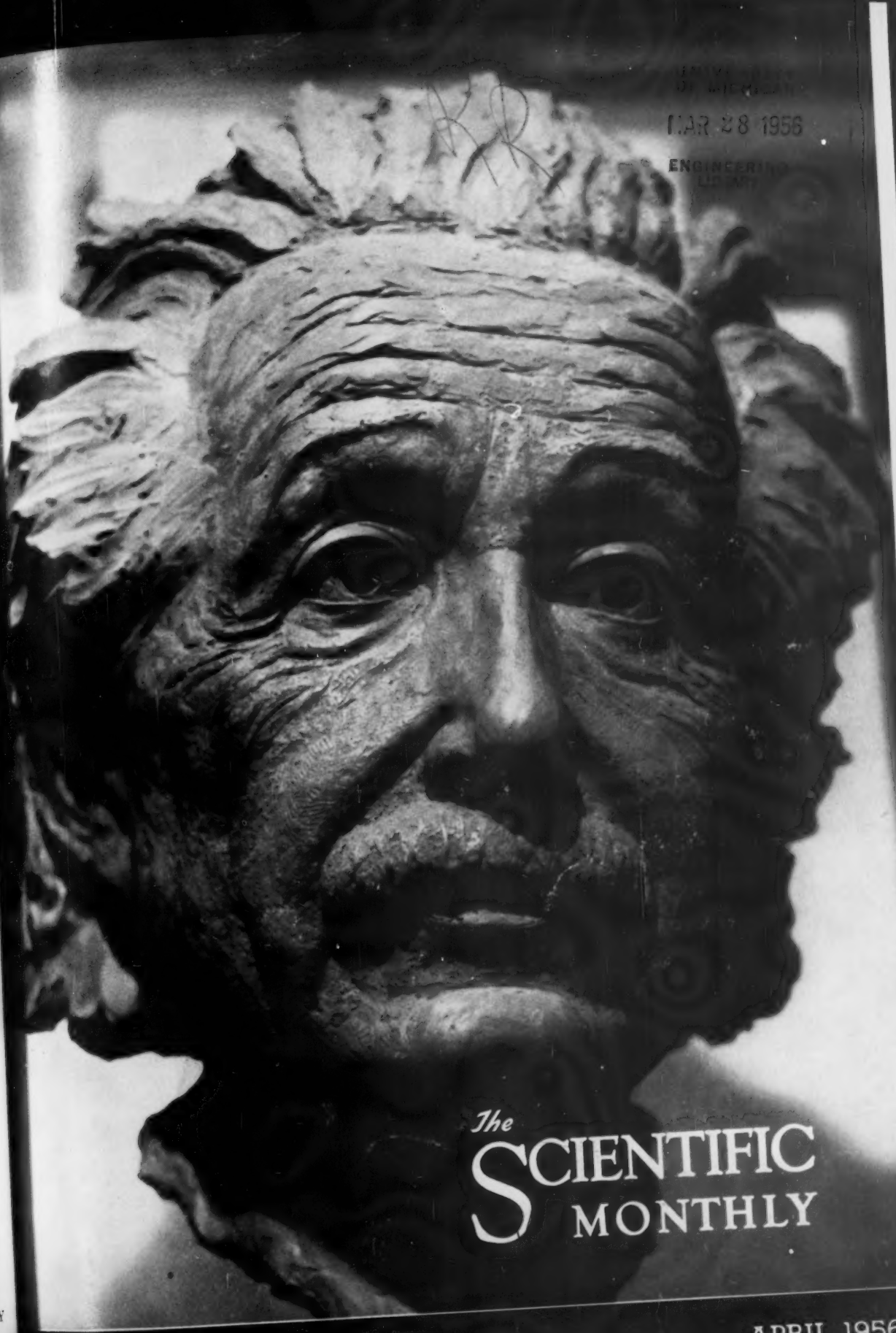
ENGINEERING



The SCIENTIFIC MONTHLY

APRIL 1956

L. 82 NO. 4

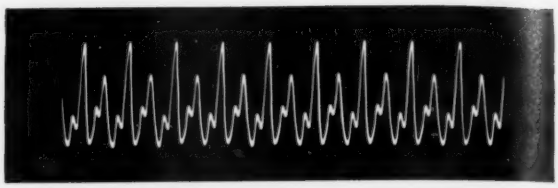
An intrstng exprmnt in spch

Some day your voice may travel by a sort of electronic "shorthand" when you telephone. Bell Laboratories scientists are experimenting with a technique in which a sample is snipped off a speech sound—just enough to identify it—and sent by wire to a receiver which rebuilds the original sound. Thus voices can be sent by means of fewer signals. More voices may economically share the wires.

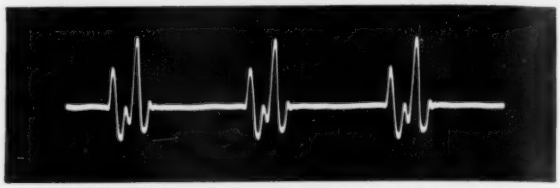
This is but one of many transmission techniques that Laboratories scientists are exploring in their search for ways to make Bell System wire and radio channels serve you more efficiently. It is another example of the Bell Telephone Laboratories research that keeps your telephone the most advanced on earth. *The oscilloscope traces at right show how the shorthand technique works.*

BELL TELEPHONE LABORATORIES

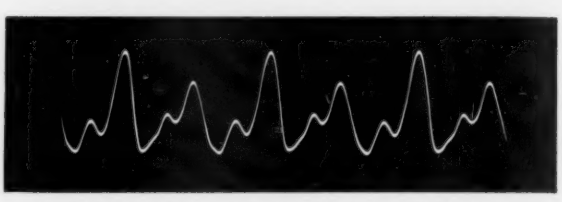
World center of communications research. Largest industrial laboratory in the United States.



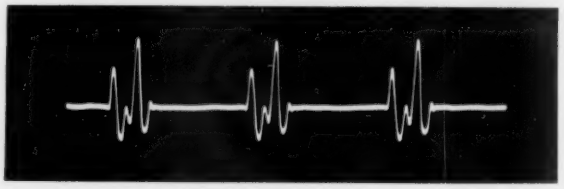
Vibrations of the sound "or" in the word "four." Pattern represents nine of the "pitch periods" which originate in puffs of air from the larynx when a word is spoken.



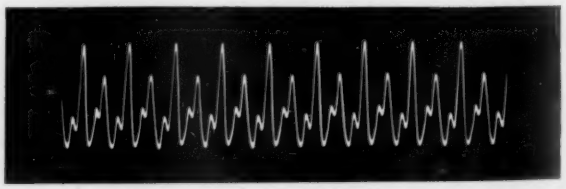
An electronic sampling of the "or" sound. One "pitch period" in three has been selected for transmission. This permits great naturalness when voice is rebuilt. Intelligible speech could be sent through a 1 in 6 sampling.



The selected samples are "stretched" for transmission. They travel in a narrower frequency band than complete sound.



Using the stretched sample as a model, the receiver restores original frequency. In all speech, sounds are intoned much longer than is needed for recognition — even by the human ear. Electronic machines perform recognition far faster than the ear.



The receiver fills in gaps between samples, recreating total original sound. Under new system, three or four voices could travel at once over a pair of wires which now carries only one — and come out clearly at the end!

THE SCIENTIFIC MONTHLY

VOL. 82 APRIL 1956 NO. 4

Principles of Mathematical Physics . . . . . *Henri Poincaré* 165

Role of Science in Marine Fisheries: Limitations and Potentialities . . . . *R. E. Coker* 176

Atmospheric Pollution and Zoning in an Urban Area . . . . . *François N. Frenkiel* 194

Radar Echoes from Birds and Insects . . . . . *Lewis L. Bonham* and *Lamont V. Blake* 204

Letters from *R. Newton Mayall, Julius Sumner Miller, Leonard Wickenden,* and *Roy W. Simonson* 210

Book Reviews of *Plant Taxonomy; Quantitative Analysis; The Botany of Cook's Voyages and Its Unexpected Significance in Relation to Anthropology, Biogeography and History; Die Binnengewässer in Natur und Kultur; Rome beyond the Imperial Frontiers; Fact, Fiction, and Forecast; Contributions to Plant Anatomy; Introduction to Social Welfare; The Unified System Concept of Nature; Advanced Calculus; Sir Joseph Banks, the Autocrat of the Philosophers, 1744–1820; The Luminescence of Biological Systems*; Books Reviewed in *Science*; New Books 213

Cover: Albert Einstein, Portrait in Bronze

[*Courtesy Emil Seletz, Beverly Hills, California, see page 175*]

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Science and Technology

(From the month's news releases; publication here does not constitute endorsement.)

Gas and Stream Analyzer

Pharmaceuticals, plastics, rubber, chemicals, petroleum, food, paper, and cosmetics can be processed automatically with a new gas and stream analyzer. The instrument is designed to be used in conjunction with narrow-band infrared ultraviolet, and visible transmission filters for studying gaseous or chemical constituents by the methods of emission, transmission, or absorption spectroscopy. (Axler Associates, Inc., Dept. SM, 102-42 43rd Ave., Corona 68, N.Y.)

Double Burette Clamp

A pair of burettes can be held parallel with a double burette clamp which has grips that hold each burette at both top and bottom. Each pair of spring-action clamps can be sprung either together or singly. The clamp is made of solid cast aluminum with black rubber-coated clamping sections. Pressure is provided by a phosphor-bronze spring. The support rod holes have a V-notch to give point support for firmly gripping a rod ½-in. in diameter. The clamp is fastened to the rod by a hand setscrew. (Central Scientific Co., Dept. SM, 1700 Irving Park Rd., Chicago, Ill.)

Paper Electrophoresis Apparatus

Continuous-flow paper electrophoresis apparatus has a new method of electrolyte feed and wide-range controls that permit the instrument to give maximum throughput under any required degree of resolution. Resolutions that formerly required as much as 1500-v operation can be made at 1/3 this value. The instrument is applicable to the separation of charged substances such as proteins, polypeptides, amino acids, and other biological materials, as well as such organic substances as dyes and intermediates. In special cases, nonpolar substances such as carbohydrates can be separated. Large-volume vertical wicks between the curtain and electrode vessels provide optimum distribution of the electric field and prevent migration of electrode products into the paper curtain. Controls permit adjustment of feed point and positioning and orientation of the pattern. (Spinco Division, Beckman Instruments, Inc., Dept. SM, 732 O'Neill Ave., Belmont, Calif.)

Photoelectric Analyzer

A new photoelectric analyzer, an instrument for automatically analyzing, recording, and controlling chemical processes involving liquids, vapors, or gases, has been announced. Automatic recording without auxiliary attendance is possible because the instrument periodically and automatically checks and resets itself to compensate for any smudge accumulation on cell windows or other similar sources of error. (Manufacturers Engineering and Equipment Corporation, Dept. SM, Hatboro, Pa.)

Camera

Automatic camera that is designed to compensate for human error and to prevent fraud simultaneously photographs an event and records on the same frame any data desired. Data are electrically transmitted to the camera from remote computing machinery or activating devices. (Taller and Cooper, Inc., Dept. SM, 75 Front St., Brooklyn 1, N.Y.)

Heat-Radiation Detectors

Series 8890 Rayotube heat-radiation detectors, which are designed for continuous temperature measurement to 5000°F and beyond, are described in a 24-page illustrated catalog that includes discussion of the theory behind heat-radiation measurements and tabulations of the characteristics of the detectors available. Catalog EN-S3. (Leeds and Northrup Co., Dept. SM, 4934 Stenton Ave., Philadelphia 44, Pa.)

Pipette Dryer

A new pipette dryer consists of a cylinder mounted on a vented base and a specially designed heater that is mounted at the lower end of the cylinder. As the hot air from the heater rises, it passes up and around and through the pipettes. Moisture is carried out through the vented top. Pipettes of any size up to 375 mm in length can be dried. The instrument is designed to take the standard rack of a 6-in. pipette washer. The rack may be taken out of the washer with its load of pipettes, drained for about 1 min, and placed in the dryer. (Phipps and Bird, Inc., Dept. SM, 6 and Byrd Streets, Richmond 5, Va.)

Acid Pump

Portable electric acid pump transfers acids from open vessels as well as from standard carboys and drums. The Centri-F pump, which is self-priming and self-draining, is furnished in materials that are suitable for use with the particular acid specified. It is driven by an enclosed electric motor and delivers a steady, spurt-free flow of acid at a rate of approximately 6 gal/min. (General Scientific Equipment Co., Dept. SM, 27 and Huntingdon Sts., Philadelphia 32, Pa.)

Telemeter Receivers

Miniature telemeter receivers, which can be used in graphic panel arrangements, have been announced by the Bristol Company. The receivers are available either as indicators or strip-chart recorders; they occupy a panel space 5 in. square. The indicators and recorders can be used to receive telemetered signals of variables such as pressure, flow, temperature, differential pressure, as well as electric measurements. Bulletin M1710. (Bristol Co., Dept. SM, Waterbury 20, Conn.)

Aerosol Sampling Instrument

Model 100B Thermopositor, an aerosol sampling instrument, is designed to offer 100-percent collection efficiency, wide particle size range, and simplicity and versatility of operation. (Roy A. Martin Co., Dept. SM, 809 Wellesley Drive N.W., Atlanta 5, Ga.)

Molybdenum Catalysts

A 24-page booklet, "Molybdenum catalysts for industrial processes," has been published by Climax Molybdenum Company. Reactions which research and pilot plant work indicate may be advantageously catalyzed by molybdenum compounds, as well as commercial processes using molybdenum catalysts, are reviewed in the booklet. The commercial reactions include oxidation, hydrogenation, dehydrogenation, isomerization, cyclization, chlorination, and condensation. Those not yet on a production basis include dehydration, polymerization, and alkylation. In addition to a discussion of each type of reaction, the booklet contains a table of typical reactions and conditions, and one section of the booklet is devoted to the factors that must be considered in choosing a specific molybdenum catalyst. (Climax Molybdenum Co. Dept. L., SM, 500 Fifth Ave., New York 36.)

Polyethylene Products

Beakers and graduated cylinders made of polyethylene are now being produced by American Agile. Use of polyethylene, the company reports, assures products free from foreign matter, thereby assuring chemical resistance to most acids, alkalies, salt solutions, and a large number of organic chemicals, all at temperatures up to 175°F. The beakers and cylinders are leakproof. Some sizes are of one-piece molded construction; others are molded, then welded. Standard beaker sizes range from 250-ml to 4000-ml capacity. Standard sizes of the graduated cylinders range from 25-ml to 1000-ml capacity. (American Agile Corp., Dept. SM, P. O. Box 168, Bedford, Ohio)

Pipette Rinser

Automatic pipette rinser made completely from polyethylene is now available. This rinser is designed so that it will fill and drain in less than 2 min. The inlet is serrated to hold a flexible tube securely; the outlet is fitted with a standard ¾-in. pipe female ell so that either a permanent drain connection or a piece of flexible hose may be used. The rinser is designed for use with the Nalgene polyethylene pipette holder. (Nagle Co., Inc., Dept. SM, Rochester 2, N.Y.)

Infrared Analyzer

A new dual-beam, nondispersive infrared analyzer employs selective detectors. The two detectors, which can be independently sensitized, are filled with gas that absorbs radiation only in the wavelength region that is useful in the analysis. The signal-to-noise ratio is high. (Applied Physics Corp., Dept. SM, 362 W. Colorado St., Pasadena, Calif.)

Spectrum Analyzer

The Todd Spectranal, a spectrum analyzer that is now being manufactured by Fisher, is available in two new models. One is equipped with spectroscope, excitation equipment, and power supply; the other has no optical equipment. (Fisher Scientific, Dept. SM, 717 Forbes St., Pittsburgh 19, Pa.)

Sterilizer Control

The Steromaster sterilizer control, can be installed on any make or type of sterilizer. The instrument maintains any temperature from 212° to 300°F within ± 1° for a specified time, automatically starts up the drying time, cuts off the steam, and indicates the progress of the cycle. The only manual operations consist of loading and dial-setting. (Consolidated Machine Corp., Dept. SM, 39 Sudbury St., Boston, Mass.)

High-Vacuum Furnace

New model of a high-vacuum furnace for use in melting, alloying, casting, and other metallurgical operations is available. The vacuum chamber is a Pyrex or stainless steel bell jar that is mounted on a polished, chrome-plated steel, vertical base plate. The pumping system permits operation in the 10^{-5}-mm-Hg range; the tungsten resistance-heating element and power supply are designed to operate at temperatures up to 2000°C. (National Research Corp., Equipment Div., Dept. SM, 160 Charlemont St., Newton Highlands 61, Mass.)

Hydrogen Analyzer

Determination of the hydrogen content of titanium, zirconium, and similar metals and alloys can be made with a new hydrogen analyzer that utilizes ¼-g samples in any solid form. The apparatus, which has a range from 5 to 700 parts per million, works on the principle of hot extraction of hydrogen in a vacuum. The standard analyzer uses a recording Pirani gage to measure and record on a strip chart the hydrogen evolved from a sample. (National Research Corp., Equipment Div., Dept. SM, 160 Charlemont St., Newton Highlands 61, Mass.)

Electronic Relay

Simultaneous operation of normally closed and normally open circuits, without switch-over, is possible with a new electronic relay. All outlets are switched and equipped with pilot lights. A control current of 3 μa actuates power loads up to 15 amp. (Forma-Scientific, Inc., Dept. SM, Box 332, Marietta, Ohio)

Silicone Reference Guide

Almost 150 of the most generally used silicone products, 18 of which were first introduced during 1955, are described in the "Silicone reference guide" for 1956. The products are grouped by physical form and cross-indexed by usage. (Dow Corning Corp., Dept. SM, Midland, Mich.)

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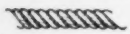

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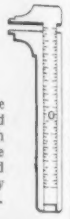

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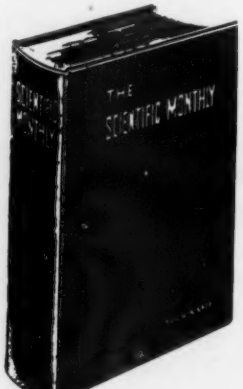

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THE
SCIENTIFIC MONTHLY

APRIL 1956

Principles of Mathematical Physics

HENRI POINCARÉ

At the beginning of the present century could anyone have predicted the striking developments in physics soon to take place? One man came surprisingly close. He was Henri Poincaré (1854–1912), professor at the Sorbonne in Paris. In 1904, a year before the publication by Einstein of his fundamental papers on special relativity, quantum theory, and the Brownian movement, Poincaré examined the state of physical research and offered to the International Congress of Arts and Sciences in St. Louis what he jokingly termed his diagnosis. Foresight is a matter of genius, hindsight is something all can enjoy. On the following pages, available for comparison with what has actually happened, are Poincaré's remarkable predictions of more than half a century ago. Poincaré's own contribution to science was broad and deep, enriching pure mathematics, mathematical physics, and astronomy. His writings on the meaning and methods of science are now accepted as classics of philosophy. The St. Louis address on "The principles of mathematical physics," translated by George Bruce Halsted, was published in The Monist, *January 1905; it served subsequently as the basis for several chapters in Poincaré's* The Value of Science.

WHAT is the actual state of mathematical physics? What are the problems it is led to set itself? What is its future? Is its orientation on the point of modifying itself?

Will the aim and the methods of this science appear in ten years to our immediate successors in the same light as to ourselves; or, on the contrary, are we about to witness a profound transformation? Such are the questions we are forced to raise in entering today upon our investigation.

If it is easy to propound them, to answer is difficult.

If we feel ourselves tempted to risk a prognostication, we have, to resist this temptation, only to think of all the stupidities the most eminent savants of a hundred years ago would have uttered, if one had asked them what the science of the nineteenth century would be. They would have believed themselves bold in their predictions, and after the event, how very timid we should have found them.

Do not, therefore, expect of me any prophecy; if I had known what one will discover tomorrow, I would long ago have published it to secure me the priority.

But if, like all prudent physicians, I shun giving a prognosis, nevertheless I cannot dispense with a little diagnostic; well, yes, there are indications of a serious crisis, as if we should expect an approaching transformation.

We are assured that the patient will not die of it, and even we can hope that this crisis will be salutary, that it was even necessary for his development. This the history of the past seems to guarantee us.

This crisis in fact is not the first, and for its comprehension it is important to recall those which have preceded it.

Mathematical physics, we know, was born of celestial mechanics, which engendered it at the end of the eighteenth century, at the moment when it itself attained its complete development. During its first years especially, the infant resembled in a striking way its mother.

The astronomic universe is formed of masses,

very great without doubt, but separated by intervals so immense, that they appear to us only as material points. These points attract each other in the inverse ratio of the square of the distances, and this attraction is the sole force which influences their movements. But if our senses were sufficiently subtle to show us all the details of the bodies which the physicist studies, the spectacle we should there discover would scarcely differ from what the astronomer contemplates. There also we should see material points, separated one from another by intervals, enormous in relation to their dimensions, and describing orbits following regular laws.

These infinitesimal stars are the atoms. Like the stars properly so called, they attract or repel each other, and this attraction or this repulsion, directed following the straight line which joins them, depends only on the distance. The law according to which this force varies as a function of the distance is perhaps not the law of Newton, but it is an analogous law; in place of the exponent – 2, we probably have a different exponent, and it is from this change of exponent that springs all the diversity of physical phenomena, the variety of qualities and of sensations, all the world colored and sonorous which surrounds us—in a word, all nature.

Such is the primitive conception in all its purity. It only remains to seek in the different cases what value should be given to this exponent in order to explain all the facts. It is on this model that Laplace, for example, constructed his beautiful theory of capillarity; he regards it only as a particular case of attraction, or as he says of universal gravitation, and no one is astonished to find it in the middle of one of the five volumes of the *Mécanique céleste*.

More recently Briot believed he had penetrated the final secret of optics in demonstrating that the atoms of ether attract each other in the inverse ratio of the sixth power of the distance; and Maxwell, Maxwell himself, does he not say somewhere that the atoms of gases repel each other in the inverse ratio of the fifth power of the distance? We have the exponent – 6, or – 5 in place of the exponent—2, but it is always an exponent.

Among the theories of this epoch, one alone is an exception, that of Fourier; in it are indeed atoms, acting at a distance one upon the other; they mutually transmit heat, but they do not attract, they never budge. From this point of view, the theory of Fourier must have appeared to the eyes of his contemporaries, to those of Fourier himself, as imperfect and provisional.

This conception was not without grandeur; it was seductive, and many among us have not finally renounced it; they know that one will attain the ultimate elements of things only by patiently disentangling the complicated skein that our senses give us; that it is necessary to advance step by step, neglecting no intermediary; that our fathers were wrong in wishing to skip stations; but they believe that when one shall have arrived at these ultimate elements, there again will be found the majestic simplicity of celestial mechanics.

Neither has this conception been useless; it has rendered us an inestimable service, since it has contributed to make precise in us the fundamental notion of the physical law.

I will explain myself; how did the ancients understand law? It was for them an internal harmony, static, so to say, and immutable; or it was like a model that nature constrained herself to imitate. A law for us is no more that at all; it is a constant relation between the phenomenon of today and that of tomorrow; in a word, it is a differential equation.

Behold the ideal form of physical law; well, it is the law of Newton which first covered it; and then how has one acclimated this form in physics; precisely in copying as much as possible this law of Newton—that is, in imitating celestial mechanics.

Nevertheless, a day arrived when the conception of central forces no longer appeared sufficient, and this is the first of those crises of which I just now spoke.

What did one do then? One gave up trying to penetrate into the detail of the structure of the universe, to isolate the pieces of this vast mechanism, to analyze one by one the forces which put them in motion, and was content to take as guides certain general principles which have precisely for object to spare us this minute study.

How so? Suppose that we have before us any machine; the initial wheel work and the final wheel work alone are visible, but the transmission, the intermediary wheels by which the movement is communicated from one to the other are hidden in the interior and escape our view; we do not know whether the communication is made by gearing or by belts, by connecting rods or by other dispositives.

Do we say that it is impossible for us to understand anything about this machine so long as we are not permitted to take it to pieces? You know well we do not, and that the principle of the conservation of energy suffices to determine for us the most interesting point. We easily ascertain that the final wheel turns ten times less quickly than the initial wheel, since these two wheels are visible; we are able thence to conclude that a couple ap-

plied to the one will be balanced by a couple ten times greater applied to the other. For that there is no need to penetrate the mechanism of this equilibrium and to know how the forces compensate each other in the interior of the machine; it suffices to be assured that this compensation cannot fail to occur.

Well, in regard to the universe, the principle of the conservation of energy is able to render us the same service. This is also a machine, much more complicated than all those of industry, and of which almost all the parts are profoundly hidden from us; but in observing the movement of those that we can see, we are able, aiding ourselves by this principle, to draw conclusions which remain true whatever may be the details of the invisible mechanism which animates them.

The principle of the conservation of energy, or the principle of Mayer, is certainly the most important, but it is not the only one; there are others from which we are able to draw the same advantage. These are the principle of Carnot, or the principle of the degradation of energy; the principle of Newton, or the principle of the equality of action and reaction; the principle of relativity, according to which the laws of physical phenomena should be the same, whether for an observer fixed, or for an observer carried along in a uniform movement of translation; so that we have not and could not have any means of discerning whether or not we are carried along in such a motion; the principle of the conservation of mass, or principle of Lavoisier. I would add the principle of least action.

The application of these five or six general principles to the different physical phenomena is sufficient for our learning of them what we could reasonably hope to know of them.

The most remarkable example of this new mathematical physics is, beyond contradiction, Maxwell's electromagnetic theory of light.

We know nothing as to what is the ether, how its molecules are disposed, whether they attract or repel each other; but we know that this medium transmits at the same time the optical perturbations and the electrical perturbations; we know that this transmission should be made conformable to the general principles of mechanics and that suffices us for the establishment of the equations of the electromagnetic field.

These principles are results of experiments boldly generalized; but they seem to derive from their generality itself an eminent degree of certitude.

In fact, the more general they are, the more frequently one has the occasion to check them, and the verifications, in multiplying themselves, in taking forms the most varied and the most unexpected, finish by leaving no longer place for doubt.

Such is the second phase of the history of mathematical physics and we have not yet emerged from it.

Do we say that the first has been useless? that during fifty years science went the wrong way, and that there is nothing left but to forget so many accumulated efforts that a vicious conception condemned in advance to nonsuccess?

Not the least in the world.

Do you believe that the second phase could have come into existence without the first?

The hypothesis of central forces contained all the principles; it involved them as necessary consequences; it involved both the conservation of energy and that of masses, and the equality of action and reaction; and the law of least action, which would appear, it is true, not as experimental verities, but as theorems and of which the enunciation would have at the same time a something more precise and less general than under their actual form.

It is the mathematical physics of our fathers which has familiarized us little by little with these divers principles; which has habituated us to recognize them under the different vestments in which they disguise themselves. One has compared them to the data of experience, or has seen how it was necessary to modify their enunciation to adapt them to these data; thereby they have been enlarged and consolidated.

So one has been led to regard them as experimental verities; the conception of central forces became then a useless support, or rather an embarrassment, since it made the principles partake of its hypothetical character.

The frames have not therefore broken, because they were elastic; but they have enlarged; our fathers, who established them, did not work in vain, and we recognize in the science of today the general traits of the sketch which they traced.

Are we about to enter now upon the eve of a second crisis? These principles on which we have built all—are they about to crumble away in their turn? Since some time, this may well be asked.

In hearing me speak thus, you think without doubt of radium, that grand revolutionist of the present time, and in fact I will come back to it presently; but there is something else.

It is not alone the conservation of energy which is in question; all the other principles are equally in danger, as we shall see in passing them successively in review.

Let us commence with the principle of Carnot.

This is the only one which does not present itself as an immediate consequence of the hypothesis of central forces; more than that, it seems, if not to directly contradict that hypothesis, at least not to be reconciled with it without a certain effort.

If physical phenomena were due exclusively to the movements of atoms whose mutual attraction depended only on the distance, it seems that all these phenomena should be reversible; if all the initial velocities were reversed, these atoms, always subjected to the same forces, ought to go over their trajectories in the contrary sense, just as the earth would describe in the retrograde sense this same elliptic orbit which it describes in the direct sense, if the initial conditions of its movement had been reversed. On this account, if a physical phenomenon is possible, the inverse phenomenon should be equally so, and one should be able to reascend the course of time.

But it is not so in nature, and this is precisely what the principle of Carnot teaches us; heat can pass from the warm body to the cold body; it is impossible afterwards to make it reascend the inverse way and reestablish differences of temperature which have been effaced.

Motion can be wholly dissipated and transformed into heat by friction; the contrary transformation can never be made except in a partial manner.

We have striven to reconcile this apparent contradiction. If the world tends toward uniformity, this is not because its ultimate parts, at first unlike, tend to become less and less different; it is because, shifting at hazard, they end by blending. For an eye which should distinguish all the elements, the variety would remain always as great, each grain of this dust preserves its originality and does not model itself on its neighbors; but as the blend becomes more and more intimate, our gross senses perceive no more than the uniformity. Behold why, for example, temperatures tend to a level, without the possibility of turning backwards.

A drop of wine falls into a glass of water; whatever may be the law of the internal movements of the liquid, we soon see it colored of a uniform rosy tint and from this moment, one may well shake the vase, the wine and the water do not seem able any more to separate. See, thus, what would be the type of the reversible physical phenomenon: to hide a grain of barley in a cup of wheat, this is easy; afterwards to find it again and get it out, this is practically impossible.

All this Maxwell and Boltzmann have explained; the one who has seen it most clearly, in a book too little read because it is a little difficult to read, is Gibbs, in his *Elementary Principles of Statistical Mechanics.*

For those who take this point of view, the principle of Carnot is only an imperfect principle, a sort of concession to the infirmity of our senses; it is because our eyes are too gross that we do not distinguish the elements of the blend; it is because our hands are too gross that we cannot force them to separate; the imaginary demon of Maxwell, who is able to sort the molecules one by one, could well constrain the world to return backward. Can it return of itself? That is not impossible; that is only infinitely improbable.

The chances are that we should long await the concourse of circumstances which would permit a retrogradation, but soon or late, they would be realized, after years whose number it would take millions of figures to write.

These reservations, however, all remained theoretic and were not very disquieting, and the principle of Carnot retained all its practical value.

But here the scene changes.

The biologist, armed with his microscope, long ago noticed in his preparations disorderly movements of little particles in suspension; this is the Brownian movement; he first thought this was a vital phenomenon, but soon he saw that the inanimate bodies danced with no less ardor than the others; then he turned the matter over to the physicists. Unhappily, the physicists remained long uninterested in this question; one concentrates the light to illuminate the microscopic preparation, thought they; with light goes heat; thence inequalities of temperature and in the liquid interior currents which produce the movements of which we speak.

M. Gouy had the idea to look more closely, and he saw or thought he saw that this explanation is untenable, that the movements become more brisk as the particles are smaller, but that they are not influenced by the mode of illumination.

If then these movements never cease, or rather are reborn without cease, without borrowing anything from an external source of energy, what ought we to believe? To be sure, we should not renounce our belief in the conservation of energy, but we see under our eyes now motion transformed into heat by friction, now heat changed inversely into motion, and that without loss since the movement lasts forever. This is the contrary of the principle of Carnot.

If this be so, to see the world return backward, we no longer have need of the infinitely subtle eye of Maxwell's demon; our microscope suffices us. Bodies too large—those, for example, which are a

tenth of a millimeter—are hit from all sides by moving atoms, but they do not budge, because these shocks are very numerous and the law of chance makes them compensate each other; but the smaller particles receive too few shocks for this compensation to take place with certainty and are incessantly knocked about. And behold already one of our principles in peril.

We come to the principle of relativity: this not only is confirmed by daily experience, not only is it a necessary consequence of the hypothesis of central forces, but it is imposed in an irresistible way upon our good sense, and yet it also is battered.

Consider two electrified bodies; though they seem to us at rest, they are both carried along by the motion of the earth; an electric charge in motion, Rowland has taught us, is equivalent to a current: these two charged bodies are, therefore, equivalent to two parallel currents of the same sense and these two currents should attract each other. In measuring this attraction, we measure the velocity of the earth—not its velocity in relation to the sun or the fixed stars, but its absolute velocity.

I well know what one will say—it is not its absolute velocity that is measured, it is its velocity in relation to the ether. How unsatisfactory that is! Is it not evident that from the principle so understood we could no longer get anything? It could no longer tell us anything just because it would no longer fear any contradiction.

If we succeed in measuring anything, we would always be free to say that this is not the absolute velocity in relation to the ether, it might always be the velocity in relation to some new unknown fluid with which we might fill space.

Indeed, experience has taken on itself to ruin this interpretation of the principle of relativity; all attempts to measure the velocity of the earth in relation to the ether have led to negative results. This time experimental physics has been more faithful to the principle than mathematical physics; the theorists, to put in accord their other general views, would not have spared it; but experiment has been stubborn in confirming it.

The means have been varied in a thousand ways and finally Michelson has pushed precision to its last limits; nothing has come of it. It is precisely to explain this obstinacy that the mathematicians are forced today to employ all their ingenuity.

Their task was not easy, and if Lorentz has gotten through it, it is only by accumulating hypotheses. The most ingenious idea has been that of local time.

Imagine two observers who wish to adjust their watches by optical signals; they exchange signals, but as they know that the transmission of light is not instantaneous, they take care to cross them.

When the station B perceives the signal from station A, its clock should not mark the same hour as that of the station A at the moment of sending the signal, but this hour augmented by a constant representing the duration of the transmission. Suppose, for example, that the station A sends its signal when its clock marks the hour o, and that the station B perceives it when its clock marks the hour t. The clocks are adjusted if the slowness equal to t represents the duration of the transmission, and to verify it, the station B sends in its turn a signal when its clock marks o; then the station A should perceive it when its clock marks t. The timepieces are then adjusted. And in fact, they mark the same hour at the same physical instant, but on one condition, which is that the two stations are fixed. In the contrary case, the duration of the transmission will not be the same in the two senses, since the station A, for example, moves forward to meet the optical perturbation emanating from B, while the station B flies away before the perturbation emanating from A. The watches adjusted in that manner do not mark, therefore, the true time —they mark what one may call the *local time*, so that one of them goes slow on the other. It matters little since we have no means of perceiving it. All the phenomena which happen at A, for example, will be late, but all will be equally so, and the observer who ascertains them will not perceive it since his watch is slow; so as the principle of relativity would have it, he will have no means of knowing whether he is at rest or in absolute motion.

Unhappily, that does not suffice, and complementary hypotheses are necessary; it is necessary to admit that bodies in motion undergo a uniform contraction in the sense of the motion. One of the diameters of the earth, for example, is shrunk by 1/200,000,000 in consequence of the motion of our planet, while the other diameter retains its normal length. Thus, the last little differences find themselves compensated. And, then, there still is the hypothesis about forces. Forces, whatever be their origin, gravity as well as elasticity, would be reduced in a certain proportion in a world animated by a uniform translation; or, rather, this would happen for the components perpendicular to the translation; the components parallel would not change.

Resume, then, our example of two electrified bodies; these bodies repel each other, but at the same time if all is carried along in a uniform trans-

lation, they are equivalent to two parallel currents of the same sense which attract each other. This electrodynamic attraction diminishes, therefore, the electrostatic repulsion, and the total repulsion is more feeble than if the two bodies were at rest. But since to measure this repulsion we must balance it by another force, and all these other forces are reduced in the same proportion, we perceive nothing.

Thus, all is arranged, but are all the doubts dissipated?

What would happen if one could communicate by nonluminous signals whose velocity of propagation differed from that of light? If, after having adjusted the watches by the optical procedure, one wished to verify the adjustment by the aid of these new signals, then would appear divergences which would render evident the common translation of the two stations. And are such signals inconceivable, if we admit with Laplace that universal gravitation is transmitted a million times more rapidly than light?

Thus, the principle of relativity has been valiantly defended in these latter times, but the very energy of the defense proves how serious was the attack.

Let us speak now of the principle of Newton, on the equality of action and reaction.

This is intimately bound up with the preceding, and it seems indeed that the fall of the one would involve that of the other. Thus we should not be astonished to find here the same difficulties.

Electrical phenomena, we think, are due to the displacements of little charged particles, called electrons, immersed in the medium that we call ether. The movements of these electrons produce pertubations in the neighboring ether; these perturbations propagate themselves in every direction with the velocity of light, and in turn other electrons, originally at rest, are made to vibrate when the perturbation reaches the parts of the ether which touch them.

The electrons, therefore, act on one another, but this action is not direct—it is accomplished through the ether as intermediary.

Under these conditions can there be compensation between action and reaction, at least for an observer who should take account only of the movements of matter—that is to say, of the electrons—and who should be ignorant of those of the ether that he could not see? Evidently not. Even if the compensation should be exact, it could not be simultaneous. The perturbation is propagated with a finite velocity; it therefore reaches the second electron only when the first has long ago entered upon its rest.

This second electron, therefore, will undergo, after a delay, the action of the first, but certainly it will not react on this, since around this first electron nothing any longer budges.

The analysis of the facts permits us to be still more precise. Imagine, for example, a Hertzian generator, like those employed in wireless telegraphy; it sends out energy in every direction; but we can provide it with a parabolic mirror, as Hertz did with his smallest generators, so as to send all the energy produced in a single direction.

What happens then according to the theory? It is that the apparatus recoils as if it were a gun and as if the energy it has projected were a bullet; and that is contrary to the principle of Newton, since our projectile here has no mass, it is not matter, it is energy.

It is still the same, moreover, with a beacon light provided with a reflector, since light is nothing but a perturbation of the electromagnetic field. This beacon light should recoil as if the light it sends out were a projectile. What is the force that this recoil should produce? It is what one has called the Maxwell-Bartholdi pressure. It is very minute, and it has been difficult to put it into evidence even with the most sensitive radiometers; but it suffices that it exists.

If all the energy issuing from our generator falls on a receiver, this will act as if it had received a mechanical shock, which will represent in a sense the compensation of the recoil of the generator: the reaction will be equal to the action, but it will not be simultaneous; the receiver will move on but not at the moment when the generator recoils. If the energy propagates itself indefinitely without encountering a receiver, the compensation will never be made.

Does one say that the space which separates the generator from the receiver and which the perturbation must pass over in going from the one to the other is not void, that it is full not only of ether, but of air; or even in the interplanetary spaces of some fluid subtle but still ponderable; that this matter undergoes the shock like the receiver at the moment when the energy reaches it, and recoils in its turn when the perturbation quits it? That would save the principle of Newton, but that is not true.

If energy in its diffusion remained always attached to some material substratum, then matter in motion would carry along light with it, and Fizeau has demonstrated that it does nothing of the sort, at least for air. This is what Michelson and Morley have since confirmed.

One may suppose also that the movements of matter, properly so called, are exactly compensated

by those of the ether; but that would lead us to the same reflections as just now. The principle so extended would explain everything, since whatever might be the visible movements, we would always have the power of imagining hypothetical movements which compensated them.

But if it is able to explain everything, this is because it does not permit us to foresee anything; it does not enable us to decide between different possible hypotheses, since it explains everything beforehand. It therefore becomes useless.

And then the suppositions that it would be necessary to make on the movements of the ether are not very satisfactory.

If the electric charges double, it would be natural to imagine that the velocities of the divers atoms of ether double also, and for the compensation, it would be necessary that the mean velocity of the ether quadruple.

This is why I have long thought that these consequences of theory, contrary to the principle of Newton, would end some day by being abandoned, and yet the recent experiments on the movements of the electrons issuing from radium seem rather to confirm them.

I arrive at the principle of Lavoisier on the conservation of masses: certainly, this is one not to be touched without unsettling all mechanics.

And now certain persons think that it seems true to us only because one considers in mechanics merely moderate velocities, but that it would cease to be true for bodies animated by velocities comparable to that of light. Now these velocities, it is believed at present, they have been realized; the cathode rays or those of radium may be formed of very minute particles or of electrons which are displaced with velocities smaller no doubt than that of light, but which might be its one-tenth or one-third.

These rays can be deflected, whether by an electric field, or by a magnetic field, and we are able, by comparing these deflections, to measure at the same time the velocity of the electrons and their mass (or rather the relation of their mass to their charge). But when it was seen that these velocities approached that of light, it was decided that a correction was necessary.

These molecules, being electrified, could not be displaced without agitating the ether; to put them in motion it is necessary to overcome a double inertia, that of the molecule itself and that of the ether. The total or apparent mass that one measures is composed, therefore, of two parts: the real or mechanical mass of the molecule and the electrodynamic mass representing the inertia of the ether.

The calculations of Abraham and the experiments of Kaufmann have then shown that the mechanical mass, properly so called, is null, and that the mass of the electrons, or, at least, of the negative electrons, is of exclusively electrodynamic origin. This forces us to change the definition of mass; we cannot any longer distinguish mechanical mass and electrodynamic mass, since then the first would vanish; there is no mass other than electrodynamic inertia. But, in this case the mass can no longer be constant, it augments with the velocity, and it even depends on the direction, and a body animated by a notable velocity will not oppose the same inertia to the forces which tend to deflect it from its route, as to those which tend to accelerate or to retard its progress.

There is still a resource; the ultimate elements of bodies are electrons, some charged negatively, the others charged positively. The negative electrons have no mass, this is understood; but the positive electrons, from the little we know of them, seem much greater. Perhaps, they have, besides their electrodynamic mass, a true mechanical mass. The veritable mass of a body would, then, be the sum of the mechanical masses of its positive electrons, the negative electrons not counting; mass so defined could still be constant.

Alas, this resource also evades us. Recall what we have said of the principle of relativity and of the efforts made to save it. And it is not merely a principle which it is a question of saving, such are the indubitable results of the experiments of Michelson.

Lorentz has been obliged to suppose that all the forces, whatever be their origin, were affected with a coefficient in a medium animated by a uniform translation; this is not sufficient, it is still necessary, says he, that *the masses of all the particles be influenced by a translation to the same degree as the electromagnetic masses of the electrons.*

So the mechanical masses will vary in accordance with the same laws as the electrodynamic masses; they cannot, therefore, be constant.

Need I point out that the fall of the principle of Lavoisier involves that of the principle of Newton? This latter signifies that the center of gravity of an isolated system moves in a straight line; but if there is no longer a constant mass, there is no longer a center of gravity, we no longer know even what this is. This is why I just said that the experiments on the cathode rays appeared to justify the doubts of Lorentz on the subject of the principle of Newton.

From all these results, if they are confirmed, would arise an entirely new mechanics, which would be, above all, characterized by this fact, that

no velocity could surpass that of light, any more than any temperature could fall below the zero absolute, because bodies would oppose an increasing inertia to the causes, which would tend to accelerate their motion; and this inertia would become infinite when one approached the velocity of light.

No more for an observer carried along himself in a translation that he did not suspect could any apparent velocity surpass that of light; and this would be then a contradiction, if we recall that this observer would not use the same clocks as a fixed observer, but, indeed, clocks marking "local time."

Here we are then facing a question I content myself with stating. If there is no longer any mass, what becomes of the law of Newton?

Mass has two aspects, it is at the same time a coefficient of inertia and an attracting mass entering as factor into Newtonian attraction. If the coefficient of inertia is not constant, can the attracting mass be? That is the question.

At least, the principle of the conservation of energy yet remains to us, and this seems more solid. Shall I recall to you how it was in its turn thrown into discredit? This event has made more noise than the preceding and it is in all the memoirs.

From the first works of Becquerel, and, above all, when the Curies had discovered radium, one saw that every radioactive body was an inexhaustible source of radiations. Its activity would seem to subsist without alteration throughout the months and the years. This was already a strain on the principles; these radiations were in fact energy, and from the same morsel of radium this issued and forever issued. But these quantities of energy were too slight to be measured; at least one believed so and was not much disquieted.

The scene changed when Curie bethought himself to put radium in a calorimeter; one saw, then, that the quantity of heat incessantly created was very notable.

The explanations proposed were numerous; but in such case we cannot say, "store is no sore."

Insofar as no one of them has prevailed over the others, we cannot be sure there is a good one among them.

William Ramsay has striven to show that radium is in process of transformation, that it contains a store of energy enormous but not inexhaustible.

The transformation of radium then would produce a million times more of heat than all known transformations; radium would wear itself out in 1250 years; you see that we are at least certain to be settled on this point some hundreds of years from now. While waiting our doubts remain.

In the midst of so many ruins what remains standing? The principle of least action is hitherto intact, and Larmor appears to believe that it will long survive the others; in reality, it is still more vague and more general.

In presence of this general ruin of the principles, what attitude will mathematical physics take?

And first, before too much excitement, it is proper to ask if all that is really true. All these derogations to the principles are encountered only among infinitesimals; the microscope is necessary to see the Brownian movement; electrons are very light; radium is very rare, and one never has more than some milligrams of it at a time.

And, then, it may be asked if, beside the infinitesimal seen, there be not another infinitesimal unseen counterpoise to the first.

So, there is an interlocutory question, and, as it seems, only experiment can solve it. We have, therefore, only to hand over the matter to the experimenters, and while waiting for them to finally decide the debate, not to preoccupy ourselves with these disquieting problems, and to tranquilly continue our work, as if the principles were still uncontested. Certainly, we have much to do without leaving the domain where they may be applied in all security; we have enough to employ our activity during this period of doubts.

And as to these doubts, is it indeed true that we can do nothing to disembarrass science of them? It may be said, it is not alone experimental physics that has given birth to them; mathematical physics has well contributed. It is the experimenters who have seen radium throw out energy, but it is the theorists who have put in evidence all the difficulties raised by the propagation of light across a medium in motion; but for these it is probable we should not have become conscious of them. Well, then, if they have done their best to put us into this embarrassment, it is proper also that they help us to get out of it.

They must subject to critical examination all these new views I have just outlined before you, and abandon the principles only after having made a loyal effort to save them.

What can they do in this sense? That is what I will try to explain.

Among the most interesting problems of mathematical physics, it is proper to give a special place to those relating to the kinetic theory of gases. Much has already been done in this direction, but much still remains to be done. This theory is an eternal paradox. We have reversibility in the premises and irreversibility in the conclusions; and between the two an abyss. Statistic considerations, the law of great numbers, do they suffice to fill it? Many points still remain obscure to which it is

necessary to return, and doubtless many times. In clearing them up, we will understand better the sense of the principle of Carnot and its place in the ensemble of dynamics, and we will be better armed to properly interpret the curious experiment of Gouy, of which I have spoken.

Should we not also endeavor to obtain a more satisfactory theory of the electrodynamics of bodies in motion? It is there especially, as I have sufficiently shown, that difficulties accumulate. Evidently we must heap up hypotheses, we cannot satisfy all the principles at once; heretofore, one has succeeded in safeguarding some only on condition of sacrificing the others; but all hope of obtaining better results is not yet lost. Let us take, therefore, the theory of Lorentz, turn it in all senses, modify it little by little, and perhaps everything will arrange itself.

Thus in place of supposing that bodies in motion undergo a contraction in the sense of the motion, and that this contraction is the same whatever be the nature of these bodies and the forces to which they are otherwise submitted, could we not make a hypothesis more simple and more natural?

We might imagine, for example, that it is the ether which is modified when it is in relative motion in reference to the material medium which it penetrates, that when it is thus modified, it no longer transmits perturbations with the same velocity in every direction. It might transmit more rapidly those which are propagated parallel to the medium, whether in the same sense or in the opposite sense, and less rapidly those which are propagated perpendicularly. The wave surfaces would no longer be spheres, but ellipsoids, and we could dispense with that extraordinary contraction of all bodies.

I cite that only as an example, since the modifications, one might essay, would be evidently susceptible of infinite variation.

It is possible also that astronomy may some day furnish us data on this point; she it was in the main who raised the question in making us acquainted with the phenomenon of the aberration of light. If we make crudely the theory of aberration, we reach a very curious result. The apparent positions of the stars differ from their real positions because of the motion of the earth, and as this motion is variable, these apparent positions vary. The real position we cannot know, but we can observe the variations of the apparent position. The observations of the aberration show us, therefore, not the movement of the earth, but the variations of this movement; they cannot, therefore, give us information about the absolute motion of the earth. At least this is true in first approximation, but it would be no longer the same if we could appreciate the thousandths of a second. Then it would be seen that the amplitude of the oscillation depends not alone on the variation of the motion, variation which is well known, since it is the motion of our globe on its elliptic orbit, but on the mean value of this motion; so that the constant of aberration would not be altogether the same for all the stars, and the differences would tell us the absolute motion of the earth in space.

This, then, would be, under another form, the ruin of the principle of relativity. We are far, it is true, from appreciating the thousandths of a second, but after all, say some, the total absolute velocity of the earth may be much greater than its relative velocity with respect to the sun. If, for example, it were 300 kilometers per second in place of 30, this would suffice to make the phenomena observable.

I believe that in reasoning thus one admits a too simple theory of aberration. Michelson has shown us, I have told you, that the physical procedures are powerless to put in evidence absolute motion; I am persuaded that the same will be true of the astronomic procedures, however far one pushes precision.

However that may be, the data astronomy will furnish us in this regard will some day be precious to the physicist. While waiting, I believe, the theorists, recalling the experience of Michelson, may anticipate a negative result, and that they would accomplish a useful work in constructing a theory of aberration which would explain this in advance.

But let us come back to the earth. There also we may aid the experimenters. We can, for example, prepare the ground by studying profoundly the dynamics of electrons; not be it understood in starting from a single hypothesis, but in multiplying hypotheses as much as possible. It will be then for the physicists to utilize our work in seeking the crucial experiment to decide between these different hypotheses.

This dynamics of electrons can be approached from many sides, but among the ways leading thither is one which has been somewhat neglected, and yet this is one of those which promise us most of surprises. It is the movements of the electrons which produce the line of the emission spectra; this is proved by the phenomenon of Zeemann; in an incandescent body, what vibrates is sensitive to the magnet, therefore electrified. This is a very important first point, but no one has gone farther; why are the lines of the spectrum distributed in accordance with a regular law?

These laws have been studied by the experimenters in their least details; they are very precise and relatively simple. The first study of these dis-

tributions recalled the harmonics encountered in acoustics; but the difference is great. Not only the numbers of vibrations are not the successive multiples of one same number, but even we do not find anything analogous to the roots of those transcendental equations to which so many problems of mathematical physics conduct us: that of the vibrations of an elastic body of any form, that of the Hertzian oscillations in a generator of any form, the problem of Fourier for the cooling of a solid body.

The laws are simpler, but they are of wholly other nature, and to cite only one of these differences, for the harmonics of high order the number of vibrations tends toward a finite limit, instead of increasing indefinitely.

That has not yet been accounted for, and I believe that there we have one of the most important secrets of nature. Lindemann has made a praiseworthy attempt, but, to my mind, without success; this attempt should be renewed. Thus we will penetrate, so to say, into the inmost recess of matter. And from the particular point of view which we today occupy, when we know why the vibrations of incandescent bodies differ from ordinary elastic vibrations, why the electrons do not behave themselves like the matter which is familiar to us, we will better comprehend the dynamics of electrons and it will be perhaps more easy for us to reconcile it with the principles.

Suppose, now, that all these efforts fail, and after all I do not believe they will, what must be done? Will it be necessary to seek to mend the broken principles in giving what we French call a *coup de pouce?* That is evidently always possible, and I retract nothing I have formerly said.

Have you not written, you might say if you wished to seek a quarrel with me, have you not written that the principles, though of experimental origin, are now unassailable by experiment because they have become conventions? And now you have just told us the most recent conquests of experiment put these principles in danger. Well, formerly I was right and today I am not wrong.

Formerly I was right, and what is now happening is a new proof of it. Take for example the calorimeter experiment of Curie on radium. Is it possible to reconcile that with the principle of the conservation of energy?

It has been attempted in many ways; but there is among them one I should like you to notice.

It has been conjectured that radium was only an intermediary, that it only stored radiations of unknown nature which flashed through space in every direction, traversing all bodies, save radium, without being altered by this passage and without exercising any action upon them. Radium alone took from them a little of their energy and afterward gave it out to us in divers forms.

What an advantageous explanation, and how convenient! First, it is unverifiable and thus irrefutable. Then again it will serve to account for any derogation whatever to the principle of Mayer; it responds in advance not only to the objection of Curie, but to all the objections that future experimenters might accumulate. This energy new and unknown would serve for everything. This is just what I have said, and therewith we are shown that our principle is unassailable by experiment.

And after all, what have we gained by this *coup de pouce?*

The principle is intact, but thenceforth of what use is it?

It permitted us to foresee that in such or such circumstance we could count on such a total quantity of energy; it limited us; but now that one puts at our disposition this indefinite provision of new energy, we are limited by nothing; and, as I have written also, if a principle ceases to be fecund, experiment, without contradicting it directly, will however have condemned it.

This, therefore, is not what would have to be done, it would be necessary to rebuild anew.

If we were cornered down to this necessity, we should moreover console ourselves. It would not be necessary thence to conclude that science can weave only a Penelope's web, that it can build only ephemeral constructions, which it is soon forced to demolish from top to bottom with its own hands.

As I have said, we have already passed through a like crisis. I have shown you that in the second mathematical physics, that of the principles, we find traces of the first, that of the central forces; it will be just the same if we must learn a third.

Of such an animal as exuviates, as breaks its too narrow carapace and makes itself a fresh one, under the new envelope we easily recognize the essential traits of the organism which have subsisted.

We cannot foresee in what way we are about to expand; perhaps it is the kinetic theory of gases which is about to undergo development and serve as model to the others. Then, the facts which first appeared to us as simple, thereafter will be merely results of a very great number of elementary facts which only the laws of chance make cooperate for a common end. Physical law will then take an entirely new aspect; it will no longer be solely a differential equation, it will take the character of a statistical law.

Perhaps likewise, we should construct a whole new mechanics, that we only succeed in catching a glimpse of, where inertia increasing with the

velocity, the velocity of light would become an impassable limit.

The ordinary mechanics, more simple, would remain a first approximation, since it would be true for velocities not too great, so that one would still find the old dynamics under the new.

We should not have to regret having believed in the principles, and even, since velocities too great for the old formulas would always be only exceptional, the surest way in practice would be still to act as if we continued to believe in them. They are so useful, it would be necessary to keep a place for them. To determine to exclude them altogether would be to deprive oneself of a precious weapon. I hasten to say in conclusion we are not yet there, and as yet nothing proves that the principles will not come forth from the combat victorious and intact.

A Neurosurgeon Looks at Sculpture

This month's cover is a photograph of a portrait in bronze of Albert Einstein by Emil Seletz, which will be unveiled in the rotunda of the Jewish Community Building in Los Angeles, California, on 18 April, the first anniversary of Dr. Einstein's death. Dr. Seletz is author of *Surgery of Peripheral Nerves*. At present he is assistant professor of neurologic surgery at the University of Southern California School of Medicine, chief of neurologic surgery at Cedars of Lebanon Hospital, and senior attending neurologic surgeon to the Los Angeles County General Hospital. Describing his approach, as a physician, to sculpture, Dr. Seletz says:

"A portrait bust is a biographic sketch of an individual, recorded for posterity, in bronze or stone. But if a piece of sculpture is to have enduring value, it must possess much more than physical likeness. It must possess personality, true character, a suggestion of the subject's life, aims, purpose, state of mind, scope of thinking, strength, weaknesses and possibly, the field of his endeavor.

"The neurologic surgeon's entire life is spent in studying intimately the face of the patient, minutely observing facial expression. For while there are innumerable tests to aid in the diagnosis of some diseases of the brain and nervous system, there are still a greater number of brain affections and mental aberrations for which there are no diagnostic tests, save the physician's keen sense of observation. The sick face, surely, teaches much, for it is a face stripped of vanity, devoid of subterfuge and sham. It is a face starkly real with pain, apprehension, and the desperate need for help. These faces, more than any reading of textbooks or study of great works of art, have taught me to read character and motive in the face of my sitter. So, as my patients' faces became my textbook in medicine, with time, my sitter became my teacher in art.

"Einstein's face was both a lure and an enigma to me. I saw in that wonderful face much to record. Yet, how can a mere mortal hope, in static form, to reveal the unfathomable genius of Einstein? I worked with humility, intent more upon the grave nobility of the man than his vast mentality."

Role of Science in Marine Fisheries: Limitations and Potentialities

R. E. COKER

Dr. Coker is emeritus Kenan professor of zoology at the University of North Carolina, where he introduced courses in fresh-water and marine hydrobiology in 1923. Last year he was visiting professor of marine biology and consultant to the Institute of Marine Biology at the University of Puerto Rico. Dr. Coker has served at various times as director or acting director of several U. S. Fisheries Biological Laboratories, including those at Beaufort, North Carolina, Fairport, Iowa, and Woods Hole, Massachusetts. In addition, he has held the positions of chief of the Division of Scientific Inquiry in the U. S. Bureau of Fisheries, chairman of the North Carolina Survey of Marine Fisheries, organizing director of the Institute of Fisheries Research of the University of North Carolina, and chairman of the Division of Biology and Agriculture of the National Research Council.

DURING the past fourscore years, applications of science in agriculture have effected a veritable revolution in farming practices. We can hardly imagine an American small farmer of the most "backwoods" area who is not directly or indirectly making application of modern agricultural science for better production. Clearly, scientific research is the mainstay of modern land farming. Yet, by far the greater part of the productive surface of the earth is sea, the field of the fisherman, to whom as yet the service of science has been relatively minor. Is this the fault of the scientist—or what? The question is sometimes put: Cannot science do for crops of the oceans something like what it has done and is doing for those of the land? Put in this way, the answer is a sure negative: except in localized and semienclosed areas, the betterment of basic productivity of the sea is now beyond the reach of the scientist and technician.

The Big Question

But the practical question regarding science and fisheries requires restatement. Can marine scientists, disregarding substantially the ordinary procedures of agricultural scientists, give really valuable service to marine fisheries? The answer now becomes a clear affirmative, provided only that one does not demand immediate and spectacular results and also that there is an unclouded concept of practicable objectives and of means of attaining them through scientific research. The tasks confronting the fishery scientist are usually complex and baffling enough, as will appear from discussions in this article (*1*). Unfortunately, the fishery scientist is at a yet greater disadvantage because many of these whom he would aid, and others who control funds and facilities, demand impossibly quick returns. Apparently this attitude derives only from lack of a clear grasp of limitations and real possibilities. More widespread understanding and a greater degree of patience would promote more effective service at less expense. Needed service is hampered because some troublesome problem must be solved today and another tomorrow, while the facts are not available for proper solution of either problem.

Cultivation crops from land and exploitation yields from sea offer entirely different stories of production and harvest. Without too detailed a comparison, let us note briefly some prominent contrasts between the conditions that face agricultural and fishery scientists. To marine biologists, some of the conditions may well seem too obvious to require discussion. Nevertheless, the conditions are not always fully grasped by those who would support scientific research in aid of fisheries, and their statement in brief is requisite background for consideration of potentialities and limitations.

Fertility, texture, and water content of soil call to the agriculturalist for study and action. Corresponding conditions in the sea demand no direct action from the marine scientist, even though the marine scientist most assuredly needs to know where the fertile areas are and when and why some areas are more fertile than others. Sea water, generally, is extremely infertile as compared with good farm soils. Plants in the ocean must make the best of a comparatively poor situation with regard to fertility (and, of course, without the aid of chemical science). This they can do, being microscopic in

The marine fishery scientist is concerned with invisible crops and an immensity of water that is inches to miles in depth, unconfined, and restless from top to bottom. [Courtesy Jan Hahn, Woods Hole Oceanographic Institution].

size, having a high ratio of exposed surface to volume, and being capable also of extremely rapid multiplication.

Light is an essential basis of productivity. About 70 percent of the solar radiation that reaches the surface of the earth comes to the sea, but much of it is reflected away and lost to plants within the sea; only radiation that actually enters the water can serve in marine production. On the other hand, some of the light reflected from solid earth is still available to plants of the land. Most of the radiation that enters sea water is soon absorbed, some wavelengths more quickly than others; thus, with depth, the light changes in character as well as in intensity. At the best, plants in the sea live in a dimmer light than most plants on land. Apparently, it must be said, most of them "prefer" a measure of dimness; necessity may be the mother of preference. Only the upper 50 to 200 meters of sea water, depending on the location, have enough light for photosynthesis or basic organic production. Again, marine algae find compensation in smallness, in all-round exposure to what light there is, and in lack of need to produce the elaborate supporting and water-transporting structures that are so essential to most land plants.

A very significant difference between production by land and production by sea is in the chain or web from fertility to harvestable crop. From fertility to grain suitable for the market is a chain of one link; fertility-to-grass and grass-to-beef (or milk) add only one link to the chain. Contrast the sea! The chain simile is no longer adequate. The multitudes of minute plants produced from fertility and sunlight are not directly useful to man, nor, generally, are the tiny vegetarian animals that consume the plants; one animal eats another and is devoured by something larger in an extremely complicated fashion. Hence, instead of a simple chain, we have an almost unravelable web leading from basic fertility to the larger fish that will grace the table.

The point of real concern is that, at each link of a food chain, or at every knot of the food web, there must occur great loss with respect to the proportion of the original production that goes to make the final substance that is to be harvested. At each step, so much goes to sustain internal (metabolic)

energies; so much is lost as heat; so much is left as indigestible material; so much is taken to support the activities of just running about in search of food. It takes several pounds of algae (the minute plants, the pasturage of the sea) to make a pound of copepod, several pounds of copepod to make a pound of tiny fish or shrimp, and so on. In the end, perhaps thousands of pounds of algae were produced to make a pound of bluefish or cod for the table. Actual estimates now vary widely.

At best, close comparisons between respective efficiencies of production of meat on the farm from animals that are supplied in large measure with prepared or cultivated foods and production of meat from the sea from fish that grew in the wild, foraged "on their own," and really worked for a living are wide open to dangerous fallacies. Indeed, a very notable contrast between production from land and production from sea is that, although in both cases human energy is required for the harvest, it is only for crops of the land that the energies of man play so great a part in so many ways in making something to be harvested.

Grouping of livestock is essential to profitable exploitation. The farmer brings his livestock together, keeps them together, and cares for them; he harvests if he pleases and when he pleases; rates of growth and quality of stock are prime considerations for which he depends on science—either that of his own practice or that of the experiment stations. The fisherman accepts nature's own standards of quality and rates of growth; he depends on nature to bring his "cattle" together in an accessible place; he harvests when he can, if he can, and if there is a reasonable prospect of disposing of the catch at a profit. The big if's in the equation have more significance than first meets the eye.

The farmer's field has only two dimensions, length and width; generally, too, it is enclosed; his stock is in easy view; they can be counted and weighed before harvest. The fisherman's field has three dimensions, length, width, and depth; the unconfined stock wander through all three. Since the fisherman cannot confine his stock, he would be greatly helped if he could be told where, in the regions available to him, the fish are to be found in practicable quantity at any given time. For meeting so practical a need, he calls on the scientist to tell him how fish react to external conditions, how the physical, chemical, and biological conditions change from time to time, and thus how, with reasonable assurance of accuracy, the movements and abundance of fishes can be predicted for any particular place and time.

For a service of the nature just described, the biologist alone is not fully adequate; for understanding of the environment that governs the movements of fishes, the physicist and chemist should work with the biologist. Furthermore, since populations of fishes in the sea are not subject to close scrutiny and easy counting, resort for knowledge of numbers and sizes can be gained only by methods of sampling and approximations as to numbers of each size; because samplings and the computations based on them are full of pitfalls, the biologist must often solicit the help of the qualified statistician.

By way of general qualification, it may be said that, in the few cases where exploiters of estuaries can be true "farmers" in effect, as in cultivation of the oyster, they do manage bounded areas and exercise a measure of control over sedentary populations and quality of stock. They then have *some* of the same problems as those of the land farmer, along with, it must be emphasized, a lot of special difficulties of their own.

To conclude this cursory account of contrasts, we may ask: Debarred as he is from giving aid in basic production, and from so many other offices of the agricultural scientist, what can the student of marine fisheries do? An answer by any one person to so broad a question can be only suggestive. In any event, the answer is somewhat different, according to whether we consider partially enclosed estuarial waters or the open sea beyond the outer coast line.

Science for Estuarine Fisheries

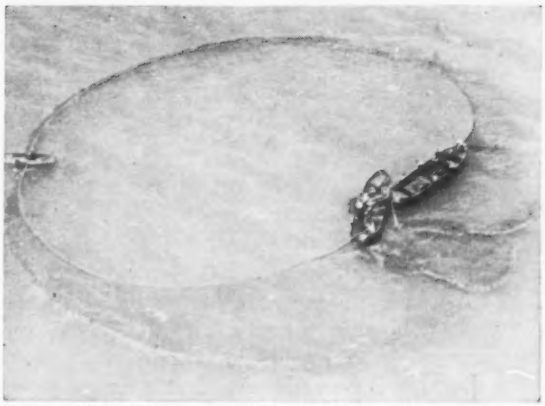
Restricted areas offer special problems. In coastal states, estuaries are important areas of fishery. Estuaries are limited in extent and highly variable in salinity and temperature, both from place to place and from time to time. They may have resident populations, but, much more significantly, they harbor migratory fishes that move in and out for spawning, for breeding, or in response to changes in the character of the water. Avenues of movement are narrow. In same cases, the objects of capture are either immobile (as the settled oyster), of slight mobility (clams), more or less lively migrants (mullet, shrimp, and blue crabs), or fleeting passengers (salmon and shad) on their way to spawning grounds far upstream in rivers or lakes. Most of these are habituated to aggregate in limited areas. Their capacities to escape capture, and thus to perpetuate the species to best advantage, are relatively small. In consequence, the populations are readily susceptible to excessive depletion through injudicious exploitation.

The danger is not that a species will be exterminated. Theoretically, it might be possible to remove

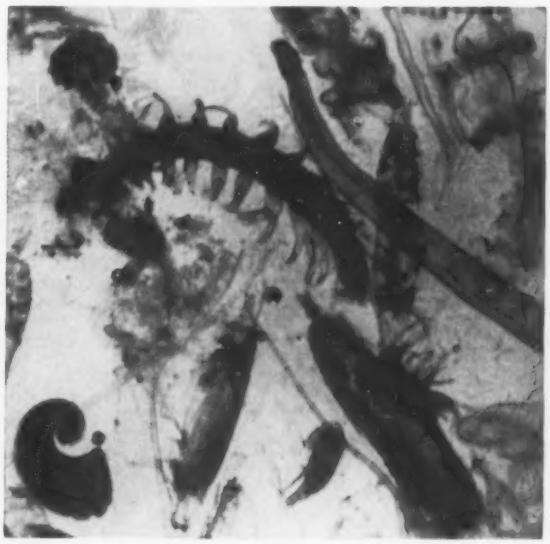
the last oyster from Long Island waters, from Chesapeake Bay, or from the North Carolina sounds, or to reduce the populations of oysters to such a degree that effective meeting of eggs and sperm would fail and the last oysters would fade out, as have some animals of the land. There is little or no danger of this. Even if, for any reason, one wanted to exterminate the oyster, the tremendous expense involved in searching out the last few potential breeders would make this impossible. The real danger is, or we should say, was (since the damage to oysters has been done) that populations will be reduced below the level of most profitable exploitation.

We know beyond peradventure that certain estuarial species—the oyster is a prime example—have been greatly depleted concomitantly with the pursuit of active exploitation. It must be said that we do not know whether other factors than the fishery may not also have played a part, perhaps sometimes the chief part, in population declines. Such other possible causes of depletion are changes in tributary rivers, alteration of the nature of bottoms and of sea inlets (worked by nature or by man), industrial and agricultural development, pollution, and even possibly innate biological cycles. Careful study and appraisal of all these external and internal conditions are tasks for the scientist and are equal in importance to the consideration of fishery practices. Nevertheless, it is quite obvious that no fish that must ascend a river to spawn can survive if passage to its natural spawning ground is completely blocked, whether by nets, dams, or pollution. It is equally certain that the oyster, which is dependent on solid bases of attachment (cultch) cannot retain its original density of population if the "rocks" (a common term for bottoms yielding oysters) are all scraped away and no new cultch is provided for the attachment of young oysters.

It seems clear, then, that for estuarial resources the maintenance of adequately productive populations depends in great part on human wisdom manifested in one way or another. Up to now, man's efforts, sensible or otherwise, toward the desired end have taken several forms. For fishes that must ascend rivers to spawn (anadromous fishes), resort to hatcheries has been general but of highly doubtful service, except for salmon whose ascent is blocked by dams that are not equipped with adequate fishways. For all estuarial fishes and shellfishes, dependence to a great extent has rested on restrictive legislation that regulates mesh size, length of nets, number of nets, periods of operation —seasonal, weekly, or hourly—sizes of boats and trawls or dredges, the use of power, and other hampering measures. Some of these restrictive regulations are doubtless necessary. It cannot be denied, however, that they are commonly based on quite inadequate knowledge of the resources it is desired to protect, of the probabilities of effectiveness of the measures adopted, or of the cost in inefficiency of operations. Admittedly they often reflect mere compromises between the demands of pressure groups influenced by competitive interests. I will consider the general principles of regulation in a later section.



Purse seine encircling a school of menhaden in waters off northern Florida [Courtesy Jan Hahn, Woods Hole Oceanographic Institution].



Plankton [Courtesy U.S. Fish and Wildlife Service].

Another form of effort for the maintenance of estuarial populations is cultivation as a public undertaking or as a field of private enterprise. Cultivation must be clearly distinguished from hatching. The eastern oyster is the best example of an estuarial resource on which great effort has been de-

voted in cultivation, or "farming." Let us consider it more particularly, remembering, of course, that on the West Coast concern is not only with the transplanted eastern oyster, but also with the "native" or western oyster and the imported "giant" Japanese oyster.

The oyster still calls for research. It has been remarked by others that the oyster has been the subject of more research than any other object of fishery; yet it is plain enough that more is needed.

The hatching of oysters, or their artificial propagation by deliberate mingling of eggs and sperm, has frequently been attempted experimentally; but it has not proved generally practicable for purposes of production, nor does it seem to be needed in most eastern estuarial waters. As a scientific venture, it may provide possibilities for obtaining useful information about natural propagation or for opening possibilities in selection or hybridizing. In the eastern states, all practical endeavors in the cultivation of oysters presuppose adequate propagation by nature somewhere. This is true whether one plants what is called "seed" (not fertilized eggs, but actual young oysters that have hatched and have grown to a fair size in nature) or lays down cultch (shells or other solid objects for the "setting" of free-swimming larval oysters).

Natural beds have been depleted almost everywhere they occur; but, long ago, it was found that oysters lend themselves well to cultivation. Oyster farming has been a profitable business when it has been established under proper conditions, as in Connecticut, New York, New Jersey, Virginia, Louisiana, and on the West Coast. In some states with waters that are fully capable of significant yield of oysters (such as Maryland and North Carolina), oyster farming has been practiced in only a small way, in comparison with seeming potentialities, yet enought to show its practicability there, too.

Because a peculiar jealousy exists respecting private control of estuarial bottoms, there have been repeated efforts in certain states to bring about restoration of natural rocks by public planting. Nowhere as yet has state planting (which is not really oyster farming) yielded really notable results. Nevertheless, it may be desirable, or at least politically necessary, to continue the attempt long enough in the present and the future to ascertain whether, under proper guidance and management, and as only a second-best resort, state planting may yield results adequate to justify the cost. Advocates of state planting have generally overlooked the fact, which is readily obvious to land farmers, that little should be expected of *planting* that is not followed by *care* or *cultivation*. *Demonstration* planting, *with cultivation,* seems to be a more worthy undertaking.

The common oyster of the eastern states, starting life as an egg discharged from the female oyster and fertilized by sperm in the outside water, develops quickly into a larva with microscopic paddles. By the active use of the paddles, it keeps in slow movement while it is carried to and fro by currents. Great numbers are devoured by predators. At a certain stage of development, survivors settle and set by cementing themselves to some solid object that is sufficiently clean, provided that such an object is present where they settle.

Still of microscopic size, the infant oyster at this stage cannot, to any extent, seek out a proper object of attachment. It may "land" in the mud, only to be smothered and lost. Even hereafter, the lucky settlers face dangers from parasites, competitors, smotherers, predators, pollution, and human exploiters. Is it surprising that a mature female oyster, or one that has attained the female condition —for they change in sex—must extrude millions of eggs during one season?

For maintenance of a stable stock, only two of the millions of eggs from one oyster need come to full maturity. For even this small return, conditions must be right with respect to temperature, salinity, currents, food, parasites, associates, and predators. All of these conditions pose problems for biological and hydrographic research, which must also be concerned with questions of market value.

The value of the oyster depends on many things, including rate of growth, size, form, color, flavor, sanitary quality, and handling from rock to table. There are tasks for the biologist, the chemist, and the bacteriologist, as well as for the research economist who has to deal with costs, processing practices, and marketing. The biologist should be further concerned about potentialities in selected strains, or races, and hybridization.

Other estuarine resources and problems. Something of what has been said concerning the oyster is also applicable to clams, except that commercial clams are not fixed in position, being always capable of slow movement. The artificial cultivation of either soft clams (*Mya*) or hard clams (*Venus* in the East, *Saxidomus,* principally, in the West) (2) has never become established as a commercial enterprise. Nevertheless, indications of practicability are sufficient to justify appropriate studies and experiments. Other clams need consideration—notably, in the West, the pismo clams (*Tivela*) and razor clams (*Ensis* and *Siliqua*). In very recent years, another clam fishery has developed off the Atlantic coast from Rhode Island to Maryland. This is for the so-called "surf-clam," *Spisula* (*Mac-*

tra) solidissima, which is fished by special gear in water up to 100 feet in depth. What problems may arise are not yet apparent. Knowledge of the lives of all shellfish subject to exploitation is desirable.

The bay scallop in estuarine waters, because of its migratory habit and unpredictable occurrence, does not lend itself to cultivation. Once an important shellfish, it virtually vanished with the disappearance, from disease, of eelgrass, which harbored particularly the young scallops. There are, doubtless, possibilities in encouragement of its return.

Blue crabs, which are prominent in the market as recently molted "soft crabs" or as picked crab meat, are found only occasionally in the open sea; they prefer shallow bays, sounds, and river estuaries, where they thrive in waters ranging from ocean-salty to brackish or even fresh. Because of a sexual difference in migratory habit, populations of males and females may be apart from each other except at mating season; the young, too, may remain more or less segregated. Being of such pronounced migratory habits, the crab does not lend itself to farming operations or to more than brief retention in confinement.

Studies of the blue crab have been made principally in the Chesapeake Bay area. For other waters, information concerning densities of populations and migrations is still woefully inadequate. In some regions, utilization of blue crabs may not now be at a desirable maximum. Much remains to be learned about the natural history, migratory habits, abundance, and distribution of crabs wherever they occur as well as about possible improved modes of capture and the processing and marketing of a resource that yields relatively high prices.

On eastern, western, and southern coasts there are other kinds of edible crabs whose habits and potentialities have been little explored. The large Dungeness crab of the Pacific Coast is important. The massive-clawed stone crab of southern waters is a shellfish of peculiarly high quality, but is perhaps too limited in abundance to offer significant opportunities for development. The northern lobster and the marine crayfishes (or spiny lobsters) of Florida, the West Indies, and other areas have received much attention; as it appears now, they must be left chiefly to their own devices in propagation and growth, although they may require protection.

High among fishery products in public esteem and in commercial value are the shrimps, of which several species are harvested—for an eager market—from the coastal waters of the Atlantic Ocean and the Gulf of Mexico, while others are taken from Pacific waters. Although the eastern shrimp are generally truly marine in life and in propagation, yet for some species at least, the estuaries are favored nurseries of the young. Different species, some of which are not distinguishable to the average consumer, have diverse habits and distribution in place and season. Much too little is known about them; yet something of what in recent years has been learned about them through long-continued and painfully meticulous studies has already paid substantial dividends in dollar returns to fishermen.

The diamondback terrapin, now unimportant as an object of fishery, demands a place in the estuarial picture for its historical interest and the peculiar economic pattern of its fishery. Highly esteemed by epicures, it was at one time the most valuable fishery product per pound in large city markets. A single terrapin of no great weight—say one with an 8-inch undershell—might bring a retail price of $5 or sometimes as much as $10. While exhaustively fished, they were not prodigal in reproduction and were slow in growth, requiring maybe 20 years to attain maximum size. Depletion of populations occurred first in northern waters, then in the Delaware and Chesapeake bays, and later in North Carolina and southward. Prices in the city markets soared, particularly for terrapin presumed to have been reared in more northern waters. Incidentally, this led in some cases to the peculiar practice of shipping terrapin northward by stages, from South Carolina to North Carolina and from North Carolina to Maryland, so that they might arrive in the market as "true" Chesapeake terrapin.

In these circumstances, experiments in propagation and farming of terrapin were inaugurated by the Federal Government and the state of North Carolina. Considerable success was attained at Beaufort, N.C., and nearly a quarter of a million young terrapin were distributed widely. Private farms came into existence and seemed definitely promising of success. Unfortunately for the profitableness of these ventures, World War I and the era of "prohibition" that accompanied and followed it led to great reduction in the demand for so high-priced a market article as the diamondback. One large private farm closed quickly, but the public venture in propagation continued for nearly 30 years. On the basis of local reports and some personal observation—but without statistical check—it seems that the number of wild terrapin shows a notable increase in recent years. Nevertheless, the most noteworthy result of the hatching operations, despite more terrapin in the water, has been, *not* more terrapin on the market, but, rather, the accumulation of a considerable body of information about the natural history and propagation of terrapin.

In estuarial regions, there is also the possibility

of "farming" of finfishes in bayous or creeks that offer practicabilities of enclosure and management, including the application of supplemental fertilizing materials. The mullet (*Mugil*) particularly, has been the subject of successful farming in some places, but not apparently on this side of the Atlantic. The mullet is an exceptional fish that seems to be at home in salt to but slightly brackish or even fresh water. It can be reared in salt or fresh ponds under conditions of artificially increased fertility, and this has long been done in other countries. Whether or not this would be profitable under our conditions has not yet been determined. The mullet has an almost unique qualification as an object of cultivation in ponds. It feeds near the base of the food chain, possessing in its mouth a fine filtering apparatus that enables it to separate from the mud and water the small algae, animals and organic detritus. The food chain from raw chemicals to table meat is nearly as short as it is for the pig.

The seaweeds are not to be ignored. Many of the useful ones, notably the kelps, are more strictly marine; yet estuaries in their more saline parts have rich natural gardens of red seaweeds, some of which are utilized for preparation of merchantable products valuable to science and industry.

Significant in most estuarial waters are fishes that live the greater part of their lives in the sea but, when time for reproduction arrives, must find their way through estuaries and up the rivers. Familiar among such anadromous fishes are shad, alewives, striped bass ("rock"), some smelts and sturgeons, and most of the salmons. These are the most vulnerable of all marine fishes. As they appear seasonally and in great numbers, the narrowing passageways offer inevitable bottlenecks where their capture is easy. They may meet actual barriers, such as dams or impassable zones of pollution with excessive depletion of free oxygen, if not with toxins or gill-lacerating fibers. The behaviors and susceptibilities of the fishes present practical problems to the scientists as well as to administrators and industrialists.

The catadromous eel (*Anguilla*) matures upstream in the rivers and creeks (females) or in the estuaries, but, in the end, must go far out in the sea to start a new generation. Since it is only the very small elvers that come in from the sea to distribute themselves in inside waters, the eels in passage are less vulnerable than the larger anadromous fishes. Yet their passage upstream may be blocked by dams and brood stock may be excessively depleted through fishing or pollution. Eel farming by trapping of the immigrating elvers and retention in ponds for growth to market size has long been practiced in Europe.

Basic hydrographic studies essential. Finally, for estuaries, let it be strongly emphasized that there is need for long-continued (and not inexpensive) surveys of hydrographic and bottom conditions and their special relationships to fishery resources. We require much more understanding than we now have of how estuaries are affected by runoff from land and by conditions in the sea outside. We need more knowledge of what goes through the several inlets, outward or inward. Interchanges through inlets, bay openings, and river mouths include not only fish, shrimp, scallops, and crabs, but also volumes of water that transport dissolved salts and other chemicals, which are the basic food for plants, as well as populations of minute floating plants and animals, the basic food supply of fish and shellfish.

Studies of this nature are clearly of indirect value; yet it is only foolish to deny their practical use. They are basic to understanding, to economical and profitable pursuit of the fisheries, and to determination of need, if any, for regulation of the fisheries. Manifestly, one ought to be able to appraise the effects for which nature rather than man is responsible. Argument ought not to be required on the question whether understanding or ignorance is the better guide in regulation and development.

Science and Sea Fisheries

Nature does the farming (hatching and rearing). We are as definitely estopped from cultivation of strictly open-sea resources as we are from fertilization of the briny "soil" that supports them. Just a few decades ago, when knowledge of the sea and its resources was of the crudest sort, it was assumed, perhaps naturally, that what worked on land would work in the sea. If one wants more cotton, corn, trees, or chickens, one plants more seed or hatches more eggs. The fisherman wants more fish; hence, 75 years ago, as well as more recently, it was generally assumed that artificial fertilizing and hatching of eggs of marine fishes and planting of fry would make larger populations, or at least compensate for depletion of populations by man's fishery.

Fortunately, even from the beginning, the main objective of marine laboratories and oceanographic cruises was to gain more knowledge of fishes and other marine animals and of the conditions under which they live, grow, and multiply. If there were needed a special argument for biological and oceanographic studies, a cogent one might be found in the fact that research in these fields has effectively undermined the bland assumptions that once supported demands for more and more marine

fish hatcheries. Unfortunately, it may be added that, if argument were required in support of a program of popular education in the results of scientific research, it might rest on the fact that, notwithstanding the seemingly clear indications of science and common sense, at least one marine hatchery still operates at taxpayers' expense, apparently in response to the demand of ill-informed voters and the misinformation or helplessness of administrators and legislators. The defense that "hatcheries cost little and keep fishermen happy" may be profitable political psychology, but it is not a happy one—insofar as the practice merely regales ignorance.

In these circumstances, it is advisable to mention briefly certain elementary facts that merit wider understanding. A previously noted difference between agriculture and fisheries should be recalled. Agriculture is concerned primarily with production, secondarily with harvest. Open-sea fishery is concerned only with the harvest of a crop of natural production. Now, it is well-known that nature is not economical in production of progeny of most commercial fishes. There are reputable estimates that a cod that has lived to market size is, on the average, but one survivor of something more than 10 million eggs produced by its maternal parent! A thousand cod in the market may represent billions that started life as fertilized eggs. The taking of spawn from captured codfish and the distribution of a few million fertilized eggs, adding them to those produced in nature with a prodigality that would require astronomical figures to give even a rough estimate, is plainly waste of time and effort. If the human fish propagator were no more successful than a parental codfish, and he is probably much less so, he would have to fertilize and plant 10 million eggs to produce one adult codfish. Would that fish ever come to market? The chances seem to be against it. Presumably we capture only a small proportion of the codfishes in the sea, although it is now thought that, in particular areas of intensive fishery, a substantial part of the population at that place is removed. Until it can be shown that the numbers of spawners and eggs produced by them play a major part in the reproductive efficiency of a marine species, there is not adequate justification for great expenditure in artificial hatching of marine fishes.

In this connection, it may be noted that in some European countries there have been practical undertakings in which small fish (plaice, a small flounder) were trawled from coastal waters, where the population was dense and the growth of fish quite slow, and transplanted to other bottoms that have fewer fish and more food. It is reported that the fish thus transplanted from crowded nurseries to good fishing grounds grew faster and attained larger sizes (*3*). Doubtless this sort of expensive fish husbandry in the sea can be profitable only under unusual circumstances.

Because we must depend solely on nature to produce the crop, it follows that to gain more profit from our labors as harvesters we must rely on more efficient exploitation or wiser conservation. *Conservation* is sometimes a fight-provoking word; let us then deal first with this avenue of supposed hope.

Idea of conservation. As is well known, when argument prevails, the basic disagreement may be not so much in objectives as in understanding of the terms used. Like so many other words, *conservation* has been a much-abused term. It should never be assumed that it is merely synonymous with *preservation* and opposed to *utilization*. Even in the limited number of cases where natural areas are set aside as national monuments or where songbirds or bright flowers are protected, the basic objective is not mere inviolate preservation but rather the maintenance of educational or esthetic values instead of other less generally esteemed uses; the choice is between two modes of utilization.

I would define conservation, as applied to self-reproducing resources, as the effort to preserve a continuing stock in the interest of more effective, economical, enduring, and profitable utilization. I see it, when properly applied, as intended to serve industrial and other modes of utilization by insuring indefinite continuance at the highest possible level of continuing profit. (For general application, the word *profit,* it should be said, might require definition, but not in relation to the resources of the sea with which we are now concerned.)

There may be yet some who would adhere to the old policy of unrestrained exploitation: trusting to the unfailing operation of the law of supply and demand, let all premiums be put on the energies, daring, and resourcefulness of the exploiters. Curiously enough, in past times, when supplies of timber, minerals, soils, and water seemed (mistakenly, we now know) to be inexhaustible, this basic philosophy played a great part in the development of this country. We have to give our ancestors credit for the effective courage, ingenuity, and drive that brought about an unprecedented accumulation of wealth in the form of bank credits, if, as we now think, with regrettable reduction of basic wealth in resources.

The philosophy of unrestrained exploitation of resources of the land no longer holds commonly. Large-scale exploiters of forests for timber and pulpwood now give thought to judicious restraint, restoration, and forest management. The efficient

farmer is an out-and-out conservationist with respect to field crops, livestock, forests, game, soils, and water: he sees water conservation as linked with soil conservation and soil conservation as linked with profits. In these and many other cases, the change is not in service of self-interest so much as in conception of a more enlightened self-interest, with recognition more and more of a high degree of linkage of interests.

With respect to conservation, the principles are the same, whether the effort is made by individuals or corporations to conserve the resources of "personally" controlled areas, or by a community or state to conserve resources of areas for which the public has retained responsibility.

With regard to fishery resources, legislators and administrators are perhaps fortunate in not having to face the proponents of esthetic values—unless the voyagers in glass-bottom boats or the wearers of aqua-lungs should yet be heard from. Their formidable constituents are those who take fish for market or for sport. Nevertheless, those responsible for formulation or enforcement of measures of conservation of fishery resources can never entirely escape consideration of the conflicts of group interests, each of which must be put in the balance as fairly and intelligently as may be possible. Hardly any measure of conservation can fail to cut across the immediate interests of some group of utilizers. The seiners, the pound-netters, the drift-netters, the oystermen, the shrimpers, the sportsmen, or any other group, can easily see their own special interests as paramount.

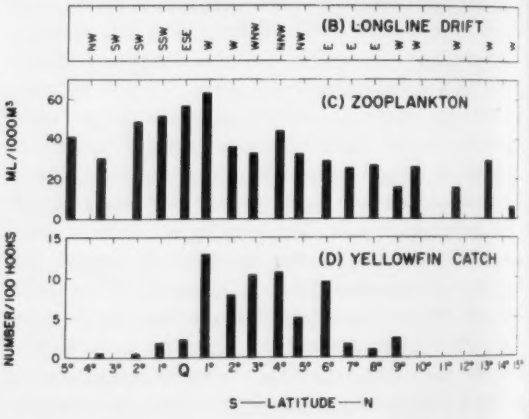
There is no debate about the desirability of maintaining perpetuity of values at the highest possible levels: the affirmative vote is unanimous. The practical question that comes to mind here is whether it is better to base exploitation and its restriction on vague guesses and general ignorance of all the complex conditions that govern the abundance and movements of fishes or on the fullest possible knowledge of the relevant facts—biological, physical, chemical, and economic. The farmer or the timber grower does not scorn the counsel and aid of trained scientists and economists; neither should the fisherman. Incidentally, it may be remarked that the landowner does not try to conserve his timber merely by requiring inefficiency of his lumberjacks; the state often does just that with respect to its fishermen.

Now, what is the general situation with respect to conservation of marine resources?

Conservation of sea fishes. It is not at all the purpose of the present discussion to propose restrictive measures in the interest of fisheries or to oppose the proposals of others. The intent here is only to emphasize the generally unrecognized complexity of the situations in which calls for regulation arise and to urge every responsible effort to break through the background of ignorance that makes for confusion and defeat. I believe that scientists should be able to help in this case as they do in others.

No one can question the fact that populations of fish and shellfish in largely enclosed sounds and river estuaries are limited in numbers of individuals and are vulnerable. If a fish of a particular kind must enter a sound or ascend a river at one or another stage of its life, it is theoretically possible to stop the last representative of the species. The qualification, theoretically, is necessary because the practicabilities will never permit so exhaustive an operation. Where areas and populations are limited, where possibilities of escape are slight for immobile or relatively inactive species (oyster, clam), where bottlenecks for active species (shad and salmon) occur here and there, restrictions of some sort become necessary.

Ideally, the objectives in restrictive regulation should be simplicity in statement, ease in enforce-



Some results of an oceanographic-fishery survey cruise in the central equatorial Pacific Ocean. Under the influences of the southeast trade winds and the rotation of the earth, the surface waters diverge near the equator and are replaced from below by cooler and more fertile waters. (*A*) The upwelling of cool water, which is indicated by cross-hatching. (*C*) Increased production of drifting organisms (the plankton) in the upwelled water, which is rich in nutrients. (*D*) The resulting concentration of yellowfin tuna, which are displaced northward as the basic food supply is carried with the surface waters in a northerly direction [Courtesy Pacific Oceanic Fishery Investigations, U.S. Fish and Wildlife Service].

ment, economy in harvesting the crop, and greatest public profit. For attainment of the ideal, regulation would necessarily be based on adequate scientific knowledge concerning the natural history of the fish, all the environmental conditions, and the relations between fish and environment, and on competent analysis of the actual results of the modes of capture that are to be regulated. The economics of the fishery also needs consideration.

The ideal seems now to be quite unattainable, not only because of the lack of requisite knowledge, but also because, in a democratic community, the conflicting interests of particular groups are not ignored—indeed, we may say, cannot be entirely ignored—even though accepted compromise may sometimes involve actual sacrifice of the greatest good to the greatest number. This admission offers no condonement for knowing sacrifice of principle. What is meant is that, in the imperfection of exact knowledge in some situations, the administrative agency may find no reasonable alternative to governance by majority opinion of those who are inadequately informed.

Nevertheless, the ideal should always be in mind; approximation to it should constantly be advocated and striven for. The requisite stores of knowledge—biological, oceanographic, technologic, and economic—should be built up with all vigor possible. If the situation with respect to regulation cannot be made perfect, certainly it is possible to make progress away from the present requirements of inefficiency in fishery.

How is it with fish of the open sea? Do we deal there with limited populations that necessitate in some cases restrictions of some sort? It matters little how many fish are in the sea with its wide extent of surface and its great depth; marine fish are of value to the commercial fisherman (or to the sports fisherman) only as they come together in such numbers and in such accessible locations that he can profitably capture them. Hence, despite the great area and immense volume of the sea, the important fisheries are localized. In general, they are not far from the coast and are on the continental shelves, where the water is not relatively deep. The more important fisheries are usually where relatively low temperatures prevail. The Grand Banks off Newfoundland, Georges Bank, the North Sea, and the halibut grounds of the northwestern states and British Columbia are well known examples.

In a preceding section it is mentioned that nutrient substance everywhere drains downward from upper to deeper waters. So it is that, wherever conditions lead to the rise of deeper, cooler and more fertile waters to the surface, or where special conditions promote active turnover of organic matter

A white marlin landed from longline gear over the stern of the *Hugh M. Smith* [Courtesy Pacific Oceanic Investigations, U.S. Fish and Wildlife Service].

in the upper, illuminated water, there occurs high production of food of fishes and, in consequence, large populations of feeders. Regions of "upwelling" off the coasts of California, Peru, and Africa are noteworthy for fish populations; so is a great region of the Antarctic south of the Antarctic Convergence, where great whale fisheries are followed; so, also, are some waters of the tropical Pacific, far from continental shores, where tuna fisheries have developed in recent years. Other more or less favorable situations are found, particularly near coasts and in estuaries.

Quite likely, all regions of potential fishery have not yet been discovered: new techniques or demands will lead to exploitation of populations and areas other than those now fished. Nevertheless, the oceans are not uniformly productive of either the food of fishes or of fishes themselves. Areas of commercial fishery are comparatively small spots in the sea as a whole.

Some investigators are led to a conclusion—supported by field studies, statistical analyses, and consideration of the economics of the fisheries—that stocks of many favored sea fishes are subject to depletion and that, throughout great areas, excessive fishery has resulted in the reduction of populations below the level of most profitable exploitation. Depletion of particular populations need not be reflected in decreased market stocks: equivalent or even increased yield may be maintained by introduction of improved methods of capture and preservation, by the use of larger vessels or trawls, or by finding new grounds, usually at greater distances and at higher cost of harvest.

From such facts and ideas, there was developed a theory or principle of the "optimum catch." The thought is that the take of a properly moderated fishery, plus natural losses, should remove only as

many pounds of fish as are added each year by new arrivals and new growth. By restriction of the fishery through voluntary action, by law, or, when necessary, through international agreement, fish populations in particular areas of fishery, such as the North Sea, Georges Bank, or the halibut grounds of the North Pacific, will be reasonably stabilized for fish of the kinds, ages, and sizes favored by the market. The populations may thus be perpetually self-sustaining at the level of most profitable exploitation, subject only to the inevitable fluctuations that result from natural causes. Increased efficiency in capture and handling would result in economy of operation and either less effort or greater profit from a fairly constant catch, or, perhaps, lower prices and increased demand—in any case, it is assumed, with advantage in profit. The theory of the optimum catch is not applicable to all species or places. The herring, for example, which is connected by only a short link to the basic food supply, is seemingly not subject to depletion.

Interest in the question of optimum catch will vary with states and regions, according to whether the harvest is taken from resident or semi-resident populations of fish in areas of limited extent or from populations in passage (migratory schools) that can be tapped only when they are temporarily within reach. The extensive literature dealing with the theory of the optimum catch has been carefully analyzed by G. L. Kesteven (*4*).

There are very real difficulties in any general application of the assumed principle of optimum catch. Many marine and anadromous fish pay no respect to state or national boundaries. Of desirable market fishes, there are a multiplicity of kinds, almost unlimited diversity of habits and ranges, and further complexities arising from year-to-year variations in ranges for particular species. In most cases, the optimum catch would have to be distributed by agreement among states or nations. At least the idea may stimulate more interstate cooperation in research to ascertain the facts.

The theory of the optimum catch and its potentialities in maintaining desirable fishery conditions, while commanding respect among many fishery biologists, is not universally accepted. There are those who seem to place paramount emphasis on natural cycles such as those that are well known to occur (independently of man's action) with terrestrial and marine animals. For any particular fish, and regardless of fishing, a series of years of successively large populations up to a certain maximum may be followed by a series of years of decreasing population. No one questions the likelihood of such cycles of natural occurrence, whether cycles of abundance are inherent in the fish or whether they are only the reflection of cyclical changes in the environment. Differences of opinion prevail only over the question whether or not major reliance can be placed on the cyclical concept to explain all variations in supply of particular fishes.

Taylor has recently presented arguments that, if accepted, would dispose of need for measures of conservation of marine fishes (*5*). Concluding that fishermen readily shift from species to species, he points out that "There do not appear to have been in the historical record any great general biological scarcities of fish. . . . The curve of total quantity of food fish produced year by year follows that of the index of industrial production which is typical of the business cycle. . . . Both the quantity and value of fish produced are determined solely by the economic factor of demand." For these data and discussion, reference must be made to the volume cited.

Whether or not one goes all the way in reliance solely on economic conditions, the emphasis on demand has importance; nothing is gained by capturing or attempting to market more fish or shellfish of any kind than consumers will buy.

On the other hand, it is hardly enough to say that the general market will always be satisfied, regardless of depletion of particular fishes of particular areas—that there are fish somewhere awaiting capture by someone with initiative and equipment. The administrator of fisheries still has to face those who may not be content that market demand is met through replacement of their own accustomed catches by other harvests. Especially is this true when the substituted fish are harvested from remote areas by those who can afford larger vessels, more expensive equipment, and more costly voyages.

Suffice it to say that no pat or generally applicable solution of the problems of actual or seeming depletion is now found in the theory of optimum catch, in the idea of cycles, or in the unfailing operation of the law of supply and demand. Some of the complexities that arise from environmental conditions or from the predator-prey relationship are suggested in the next section.

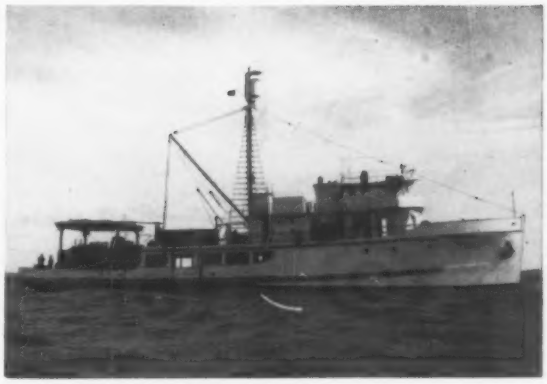
Discussion of this topic may be concluded with a quotation from Lionel A. Walford (*6*). "It is now realized that for most marine populations there is no final fixed maximum yield; that varies depending on the interactions with predation and prey populations—and with changes in environment affecting all populations. There is thus a shift of interest, in some quarters, to the study of marine *"communities"* (say all the fish populations along the Atlantic coast) and the effect of both the fishery and the environment—oceanographic and meteorological conditions—upon them. Naive acceptance

of the idea of a single "optimum catch" for all marine fisheries may well lead to disappointment in the majority of cases."

Some of the complexities—an open question. As I mentioned in a preceding section, no particular types of regulatory measures are here proposed or opposed. It is intended, rather, to point to some lines along which researches are directed or needed. It cannot be too strongly emphasized that the situation with respect to populations of marine and estuarial fishes and their interrelationships is often highly complex. Some of the complexities arise from the fact that almost all commercial fishes are predators. Each predator lives at the expense of its neighbors: at the very least it feeds with them from a common table. Insofar as a particular fish is protected, others are left unprotected from it as predator or as competitor. One winces at the destruction of young sea trout (squeteague) in the shrimp fishery. If this is restrained, what is to protect the shrimp and the small fish from destruction by the voracious and growing sea trout? If one protects the bluefish, "perhaps the most ferocious and bloodthirsty fish in the sea," one withdraws by so much protection of mackerel, sea trout, and butterfish from the rapacity of the bluefish. The mackerel, in turn, feeds in part on herring and young mackerel.

As indication that fishermen themselves have long suspected the complexity of interrelationships, I may mention (but definitely without accepting its soundness) a contention of fishermen, 80 years ago, that the invasion of bluefish into New England waters in the middle of the last century led to a fourfold increase in the population of lobsters, because the bluefish drove away mackerel, which they considered to be the greatest enemy of young lobsters (*7*). Since the mackerel is not generally a bottom feeder, we may doubt the validity of the contention. At least, however, the fishermen of an older time sensed a valid principle, whether the application in this case was right or wrong.

The existence of complicating interrelationships, which are widespread among food fishes, does not require that we be indifferent to the preventable destruction of young fish or that we regard no protection of any fish as desirable. It does mean that complex interrelationships of species come into the picture and that particular actions in restriction of a fishery may have effects that were neither contemplated nor desired. It means, above all, that sound opinions regarding protective measures should not be arrived at by offhand glances at just one part of the picture, however vivid that part may seem to be. Snap judgments for or against protection are equally unreliable.



The *Hugh M. Smith,* a 128-foot U.S. Fish and Wildlife Service research vessel that is used for exploring fishery potentials in the central equatorial Pacific Ocean [Courtesy Pacific Oceanic Fishery Investigations, U.S. Fish and Wildlife Service].

What is needed is more accurate and broader knowledge of the interrelationships of species. An objective is more intelligent appraisal of what may be called the "biological cost" of giving or withholding protection in particular cases. Surely we want also more deliberate and precise analyses of the economic cost to fishermen that is involved in either alternative. Perhaps in some cases we may never arrive at completely satisfactory answers. Nevertheless, costly errors of judgment are fewer when understanding is improved.

Service of Science

By what has been said, we are led to the inescapable conclusion that fishery science for the open sea, in contrast to agricultural science for the land, can do nothing about the crop with respect to fertility or propagation. Unless we can do something about survival, we are effectively hedged in insofar as production of the harvestable crop is concerned. Wherein, then, is the potential service of fishery science? In attempting at least a partial answer to a question that strikes at the very foundation of fisheries laboratories, we may list a few more-or-less-obvious lines of service.

In the first place, the cited limitations on human contribution to production of the crop do not, as we have seen, apply to some important shellfish of the estuarial waters. Seeding and cultivation of oysters of several species, considering east and west coasts, for increased production can yield results (with or without resort to artificial hatching). Clams of various kinds seem also to offer presently unrealized possibilities.

Anadromous fishes, such as salmons, shad, and striped bass, present special problems and, it is to be

expected, fair opportunities for promoting by proper restraints greater stability of populations and harvest. A diversity of scientific studies is needed to determine causes of fluctuations in yield. To what extent are fluctuations attributable to excessive or improper fishery? to variations in market demand? to barriers, complete or partial, caused by dams or by pollution? to innate biological cycles of reproduction and growth? to varying conditions of precipitation and discharge of streams the fish must ascend for spawning, hatching, and early growth? or even to changing conditions of circulation, nutrition, competition, or predation in the sea where the fish must grow to maturity? Requisite for a proper interpretation of apparent trends is a lot of precise biological and hydrographic knowledge gained over periods of years—and a good measure of sound judgment.

With respect to marine fishes, it should always be in mind that it is not alone production in the sea that determines the harvestable crop but rather, and primarily, the availability of what has been produced naturally for the harvester. It does not matter how many fish are produced naturally if we do not know where, when, and how to get them. Insurance of availability offers fairly wide scope for human effort in the fields of biology and oceanography to puzzle out the conditions that cause movements and aggregations. It is equally a problem of fishery engineering, where practical fisherman, biologist, oceanographer, and physical and chemical technicians must work hand in hand to devise ways of attracting fish into the nets and of keeping them in the best possible condition after they are caught.

In the line of exploration by the biologist and the oceanographer, there are possibilities in the discovery of new fishery grounds on the bottom or of new areas of fishery in the open sea.

In the realm of opportunities and techniques of capture and handling, there is constant challenge to the inventive mind reinforced with experience in fishery operations. The farmer with modern equipment scorns much of what his predecessors used a century ago. The fishermen of today are content with much of the old apparatus and many of the old techniques of capture. There have been improvements but all too few. Processing and marketing now give greater evidence of modernization. In general, resistance to change has characterized fishermen at least as much as it has any other group that works close to the bases of national economy.

The field of forecasting offers definite possibilities for reduction of wasted effort and expense in the fisheries. Long-continued studies of fishes, the food of fishes, the drifting microorganisms, temperatures, currents, and drifts do sometimes provide a basis of knowledge that permits forecasting with respect both to the movements of fishes and to good fishing years. The bane of most fisheries is the unpredictability of catch. If both farming and fishing may be described as gambling operations, a chief aim of science in these fields is to better the odds in favor of the operator.

By examination of the rings on scales or in earbones, a population of fish is sorted into year-classes. A year-class embraces all the individuals that, by proper interpretation of markings on the scales, are found to have started life in a particular year. It becomes evident that a given year was, or was not, good for hatching and survival. After lapse of the number of years required for fish of that kind to reach marketable size, a good year-class may be expected to appear in relative abundance in a succession of years while individual fish grow from year to year in size. Knowing, then, about the composition of the catch this year with respect to year-classes, one may forecast, with some

Scale of haddock 6 years old [Courtesy U.S. Fish and Wildlife Service].

assurance not only which year-classes will make up the population of the next year but also whether these classes promise large catches.

Just as one illustration of possibilities, a recent report of the Pacific Biological Station at Nanaimo, British Columbia, may be quoted. As a feature of their intensive and protracted studies of the herring, they say: "Predictions of fishing success are attempted each year after consideration of all pertinent data on each population. They make available to the fishery industry information of possible value in the efficient utilization of the resources. . . . Predictions of year-class strength, population abundance, and the relative size of fish in the various fisheries have generally been correct."

One sort of forecasting derives from prolonged studies of the physical and chemical qualities of the water and of the drifting microorganisms (the plankton). Sometimes it may be determined (on the basis of sufficient experience in a given area) approximately how far north or south, east or west, schools of migratory fish will come in a given season.

There are also modes of short-range, hour-to-hour forecasting—for example, by the "plankton indicator," the thermometer, or the echo sounder. The idea that minute drifting organisms may be indicators of the presence of prey is not new. Whalers, observing the color or the "soupiness" of surface waters, have long recognized "whale feed" (copepods, mollusks, small shrimp) and have been guided by its presence or absence. But feed and fish are not necessarily at the surface. The originality in the plankton indicator is in the design by the British scientist A. C. Hardy of a simple apparatus that is easily manipulated to reveal conditions at depths that are not visible to the observer on a vessel. A vast amount of highly technical biological and oceanographic study was necessary to accumulate the foundation of knowledge that paved the way for a simple appliance having potentialities for practical use.

A tube 3 inches in diameter and 1 foot long carries a fine-meshed silk disk as a filtering screen. Attached to a line held on the vessel and provided with vanes that assures its "diving" to a proper depth, the instrument can be repeatedly thrown from a moving vessel to sample microscopic life at a depth of several fathoms. When the plankton indicator is pulled back aboard, the screen tells the story; the fisherman needs no fancy microscope or special biological knowledge. A clean disk means little food for fish and little likelihood of herring; a brown or green disk may signify abundance of minute plants that are not generally favored by herring; a reddish disk indicates copepods—"herring feed"—and suggests that nets be shot. Obviously insofar as the indicator is reliable, it can effect savings in labor, time, and expense.

The Hardy plankton indicator [Courtesy A. C. Hardy, Oxford University].

Underwater view of a sea scallop bed on Georges Bank off Cape Cod. A 2-mile strip of ocean bottom was photographed in an effort to make a census of the sea scallop beds [Courtesy J. Artur Posgay, Woods Hole Oceanographic Institution].

When one is dealing with nature, particularly in the sea, complications are to be expected. As Hardy recognizes, there are limitations on the reliability of the indicator. If a large school of herring moves into a rich feeding ground and substantially depletes the population of copepods, the sampler

might give an unfavorable indication while herring were still abundant. Again, at certain times, the movement of the fish might be guided, not by conditions of food supply, but by conditions requisite for spawning. Fishermen who rely on the indicator cannot expect 100-percent reliability. At least, the instrument can tell them of conditions, otherwise undetectable, that are quite unfavorable to fishing operations (*8*).

This device is an example. What others may come in the future?

An understanding of the temperature relations of particular fishes, derived from extensive scientific studies, has led finally to relatively simple procedures in "fishing with thermometer." Studies of temperatures, salinity, and dissolved oxygen afford means of identifying masses of water, their origins, and their relations to the distribution of fishes. A striking illustration of the potential value to fisheries of long-term studies of hydrobiology and meteorology is afforded by the fact that the yield of mackerel for May and June in the great sound of Denmark, the Kattegat, can, it is said, be predicted from the knowledge of the frequencies of eastern and western winds during the preceding months of January to April as well as from studies of salinities in the preceding months (*9*).

Less simple than the plankton indicator is the fathometer, or echo sounder, which reveals the actual presence of fish at considerable depths. It is a triumph of physical and oceanographic sciences that is invaluable to fishermen. Hardy remarks (*8*), with reference to Scottish and English fisheries: "few drifter shippers now shoot their nets without first getting an indication of fish on their echometer."

The camera, sometimes with television, has become useful to oceanographers in exploration of deep waters, to biologists in observation of fishes in their habitats, and to fishery technologists in examination of subsurface fishing operations. As everyone knows, direct observation of fishes in coastal waters is attainable through the increasingly widespread use of self-contained diving equipment. Who knows what better understanding of the behavior of fishes may accrue?

Meteorology has been alluded to. That the weather forecaster is the fisheman's best friend requires no emphasis. However, allusion should be made to the new specialty of forecasting wave heights, both for beaches and for offshore waters, a service that offers real possibilities for economy in fishery operations.

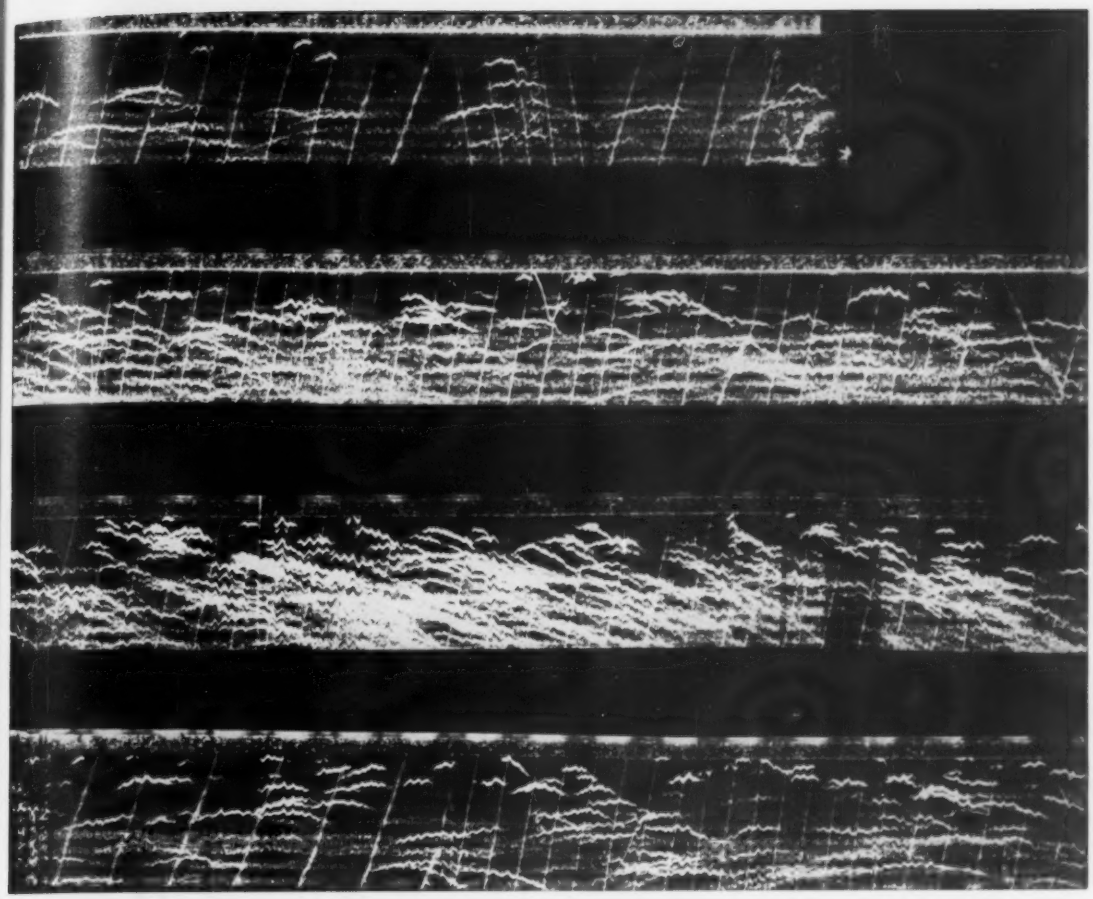
In much of what has been said about forecasting, it is assumed that we have some knowledge of the migrations of fishes and how the movements are related to temperature, plankton, and other conditions of the environment. This presupposes a lot of basic study of the natural history of fishes, gained in part by extensive tagging of individual fishes and recovery of tagged fishes. It assumes, too, a great amount of prior study of environmental conditions and of the correlation of the movements of fishes of different kinds with the environment—biological, physical, and chemical. On this, as on many fronts, the scientist cannot work alone; the cooperation of fishermen is of high importance.

Availability of fishes depends, not only on numbers and localization, but also on the behavior of fishes and their reactions with respect to modes of capture. Here, biology, physics, chemistry, and engineering come together. In some regions, lights are used extensively to attract fish to the net. This may be only a beginning. There seem to be possibilities in determining the more effective wavelengths of light. Here and there electric currents are being experimented with to direct the movements of fish or to shock them into numbness and readiness for capture. There is undoubted practical need for basic studies of the reaction of fishes to visual, electric, and chemical stimuli.

Certainly there is occasion to explore the possibilities of *new products* from the sea. During the recent world wars, there was substantial scientific and commercial activity in the extraction of a vitamin from the otherwise useless horse mussel. If the sea holds in solution the basic elements of every biological need, what may be the possibilities of extracting from the drifting microscopic plants and animals valuable food and drug products, both biotic and antibiotic?

Barring the leasing of oyster beds, there is no private ownership; fish in public waters are common property, to be taken by anyone who will pay the requisite small tax. Too often the fisheries are not conducted with the consideration for the economy of effort that must control the use of land resources, such as forests, where private holdings and control prevail. There is, and probably will always be, demand for state regulation of fishery apparatus and methods, particularly in estuarial waters; but there should be more thought of the waste in effort that is required by some of the restrictions.

It has been remarked by others that the fishery is probably the only basic industry that is repeatedly required by law to operate inefficiently and uneconomically. Perhaps some of this is practically necessary in order to spread the operations among a larger number of people. Where resources are communally owned or communally exploited, the administrator may find it hard to resist entirely the



Echo-sounder traces made with a new recording system, the sounder lowered beneath the ship's hull. The traces on successive panels, from the top down, indicate diurnal migrations of individuals or groups in a vertical direction. Greatest concentration of traces is in the third panel from the top, which was made at sunset [Courtesy Woods Hole Oceanographic Institution].

demands for rationing the rights of exploitation among the vocal groups. In any event, more serious thought should be given to the waste of effort that is required by many of the restrictions. In general, however, as has been mentioned before, the restrictions are almost universally based on paucity of knowledge, both of the fish it is desired to protect and of the economic cost of the restriction. In these conditions, it is a primary responsibility of science to ascertain and publicize the real facts of natural history and economics and thus to promote wiser regulation and greater profit. Perhaps, in some cases, scientist and economist together should design experimental regulation, competently observed and controlled.

Considering the inevitability of demands for regulation, the need to promote more economical exploitation of nature's freely produced crops, there is call for statisticians, economists, biologists, and oceanographers to work cooperatively. Biologists and oceanographers can accumulate background data on the behavior of particular fishes and shellfishes and of the physical conditions surrounding them; the objective is better understanding of the interrelationships of fish and environment. Without knowledge and understanding, the fluctuations of sizes of populations of desired fishes—or their total absence—in this place or that, at this time or another, remain impenetrable mysteries and substance for wild guesses: regulation runs amuck, and exploitation is more haphazard and less economical than need be.

Whether or not it looks "practical," what is really called for is a lot of basic biological and hydrographic study, such as has long been in progress around the British Isles, such as is being done in the California Cooperative Oceanic Fisheries Investigations, and such as is now carried on in a few other American offshore areas. Oceanographic study, it must be recognized, is not inexpensive. A

vessel must be operated; the skills of men of talent and training are required; it takes time; often, however, expense is distributed through cooperative effort among agencies of diverse specialties. Thought of long-continuing costs for ultimate economic return tends to discourage the appropriation of public funds. Yet one must emphasize the inescapable fact that investigation of almost any immediately practical problem of the fisheries is stalled sooner or later by the want of basic oceanographic knowledge that can be gained only through long-term research.

Market studies are of high importance. Better products and a broader market for them are needed. It is not enough to find good fish in the sea. Curiously enough, it is too generally the case that the fish most readily obtainable in the markets are not the fish of best natural flavor. Practices employed in capture, handling, transportation, and storage need always to keep pace with the more and more exacting requirements of the consuming public concerning appearance, grading, packing, and dependability. In actual food value, the better products of the sea rank second to no other item of food. Yet the consumption of fish in this country is quite low compared with that of other countries. In some parts of the United States, it is extremely low. What is wrong with our tastes—or with the fish we can buy? More concretely, what is wrong with the mechanism of getting from sea to table inexpensively harvested food of highest quality? Spotting a glaring deficiency in utilization of fishery resources avails little in itself. Marketing and buying practices are not easily changed. A paramount need now is for marketing research that is pursued systematically and competently by specialists in the field of economics, with the sympathies and aid of fishermen and dealers. Marketing research has paid off for agriculture; it may well do so for fisheries.

Balanced Approach

But let us for a moment be what is often called "practical-minded." A public institution for fisheries research may seem to face an impossible dilemma. It can be one of two or three things. It can become a mere political agency, playing handy man to one or another special interest of the fisheries or leaping from one piece of "research" to another as may be required to grease the wheel with loudest squeak; it accomplishes little of enduring value. With rather more stamina, it can restrict activities to "immediately practical problems," in the solution of which it runs too generally into blind alleys; the pathways are blocked by barriers of ignorance of the real facts essential to progress in solution. Assuming support, it can concentrate on basic science, leaving all hope of monetary return to some future time. Meanwhile, it needs funds: on the one hand, personnel want pay, as do equipment dealers; on the other hand, taxpayers, offered only vague suggestion of possible future repayment, are not free with credit.

Actually, with understanding leadership, the dilemmas so crudely stated need not be encountered. It is more than a matter of maintaining the golden mean. The alternatives are not mutually exclusive. The squeaks of the wheel should be heard; sometimes they point to vital problems. Immediately practical problems may be interesting and informative; they may give leads to the scientist in basic research. On the other hand, the need for basic research is not incomprehensible to the intelligent fisherman and taxpayer any more than it is to the farmer. The fishery scientist may never be able to show the spectacular economic returns of the agricultural scientist; but fisherman, as well as farmer, seeing his bread-and-butter requirements faced sympathetically and as competently as conditions permit, may well learn where the true obstacles lie, come to see his own problems in proper perspective, recognize the practical necessity of expanding the bounds of knowledge of fish and environment, and acquire a proprietary interest in the scientific endeavor. On the other hand, the scientist in a fisheries research program, if he is privileged to do more basic work, need not feel too wildly free. He is employed in a particular sort of cooperative undertaking having recognized and understandable objectives. The problem for leader and staff is to see the whole picture and to formulate a well-designed and defensible attack with diverse lines of approaches. When motives are clear, there is no more impracticality or incongruity for a fisheries laboratory than there is for an agricultural laboratory giving a *fair emphasis* to basic science.

In conclusion, it may not be quick and spectacular increases in fishery yield that can be expected but rather gradual and substantial improvement, based on scientific and economic studies to support and supplement individual initiative and enterprise, which must always be a chief reliance. Increasing profit to fishermen and to states and wiser regulation of fishery practices with fewer useless and costly restrictions are to be striven for. It is a virtual certainty that gratifying results will follow from continued and energetic cooperation between administrators, scientists, statisticians, economists, and, most important, fishery operators and fishermen.

References and Notes

1. Paper from the department of zoology and the Institute of Fisheries Research, University of North Carolina; completed in the institute of Marine Biology, University of Puerto Rico. I have gotten ideas relating to this subject where I could. Acknowledgements could be innumerable, but special mention should be made of the critical and helpful comments on earlier drafts of the manuscript that were generously given by M. R. Carriker, A. F. Chestnut, J. L. McHugh, and L. A. Walford, who, however, share no responsibility for errors and limitations that remain. Should this presentation elicit more and better thoughts from others, a major part of its purpose will have been served.

2. The hard clam, thick-shelled clam, littleneck clam, cherrystone clam, or quahog is of the genus *Venus*; the soft clam, thin-shelled clam, long neck clam, or just "clam" of northern waters is of the genus *Mya*. On the West Coast, soft clams are *Mya*, hard clams are mostly *Saxidomus*.
3. E. S. Russell, *The Overfishing Problem* (Cambridge Univ. Press, Cambridge, 1942), pp. 70 and 110.
4. G. L. Kesteven, "An examination of certain aspects of the methodology and theory of fisheries biology," mimeographed at the Bingham Oceanographic Laboratory, Yale University. (Revised from the mimeographed edition issued from Cronulla, N.S.W., in 1946.)
5. H. F. Taylor, *Survey of Marine Fisheries of North Carolina* (Univ. of North Carolina Press, Chapel Hill, 1951), pp. 320, 420, 421.
6. L. A. Walford, personal communication.
7. S. F. Baird, *Rept. of the Commissioner of Fish and Fisheries for 1871* (1873), p. 245.
8. Discussion of the plankton indicator is based in part on a personal communication (1955) from its designer, A. C. Hardy, professor of zoology, Oxford University.
9. G. L. Kesteven, referring to the effects of movements of water on abundance of mackerel in the work cited (*4*), quotes Jensen (1950) as follows: "Thus the yield of the fishery during May–June may be predicted (i) from the frequency of eastern and western winds during January–April, (ii) from the surface salinity in the Kattegat during February–April, (iii) from the bottom salinity in the Kattegat during April." Similarly, forecasts of the occurrence of mackerel in the sound in autumn are made from the meteorological-hydrographic factors of the preceding months.

The vulgar antithesis of fact and theory is founded on a misconception of the nature of scientific theory, which is, or ought to be, no more than the expression of fact in a general form. Whatever goes beyond such expression is hypothesis; and hypotheses are not ends, but means. They should be regarded as instruments by which new lines of inquiry are indicated; or by the aid of which a provisional coherence and intelligibility may be given to seemingly disconnected groups of phenomena. The most useful of servants to the man of science, they are the worst of masters. And when the establishment of the hypothesis becomes the end, and fact is alluded to only so far as it suits the 'Idee', science has no longer anything to do with the business.—T. H. HUXLEY (*The Scientific Memoirs of Thomas Henry Huxley*, M. Foster and E. R. Lankester, Eds., London, 1898–1903, vol. IV, p. 663).

Atmospheric Pollution and Zoning in an Urban Area

FRANÇOIS N. FRENKIEL

Dr. Frenkiel is a member of the principal staff of the Applied Physics Laboratory, Johns Hopkins University, where he heads the theoretical and applied mechanics group. As a result of his work on turbulent diffusion and on certain aspects of theoretical geophysics, he became interested in the application of mathematical methods to atmospheric pollution studies. His attention was drawn to the Los Angeles pollution problem by Joseph Kaplan in connection with the preparation of recommendations by the California Governor's Committee of Technical Experts on Air Pollution. Some of his ideas on atmospheric pollution were discussed at a National Science Foundation conference on the significance and possibilities of high-speed computing techniques in meteorology and oceanography that was held at the University of California, Los Angeles, in May 1954.

VARIOUS human activities are responsible for discharging pollutants into the atmosphere of an urban area. The prevailing meteorological conditions, the local topographic situation, the distribution and nature of pollution sources, and the rates of emission from such sources determine the pollution patterns over the urban area. Under some particular meteorological and topographic conditions, atmospheric pollution may become objectionable in some parts of the urban area during limited periods of time. In others, the atmospheric pollution does not attain very high concentrations at any particular time, but the pollution effects added over months or years result variously in damaged crops, soiled houses, increased morbidity and mortality, and an unesthetic appearance of the area. Finally, atmospheric pollution should be considered when the expansion of an urban area or an increase in its population is expected or planned.

Let us consider an urban area with various kinds of pollution sources such as industry, municipal and household incinerators, house heating, motor vehicles, railroads, and the inhabitants themselves. The pollutants emitted by such sources into the atmosphere include solid particles, liquid droplets, vapors, and gases. Some of the heavier particles fall out rapidly to the ground near the pollution sources, and the lighter ones deposit at some distance. However, a large amount of the pollutants move through the community before they are dispersed into the surrounding areas. Sometimes a temperature inversion confines the pollutants to lower levels of the atmosphere, or a mountain chain restricts their dispersion out of the community. After a certain time, they spread out into the rural areas, reaching other communities, another state, or even another country. The nature of some pollutants may change by chemical or physical processes that take place during their dispersion. These processes may be due to meteorological causes such as snow, rain, and fog or to solar radiation and interactions with aerosols of natural origin, including salt nuclei, volcano dust, pollen, or such gases as ozone and nitrogen oxides. Various pollutants may interact among themselves, disintegrate, or otherwise change during their dispersion. Except for those pollutants that change into normal constituents of the atmosphere, the dispersing aerosols and gases come after a certain time into contact with the surface of the earth. Most of the solid particulates reach the land or the sea as a fallout; other pollutants may be washed down by the rain or may simply hit the ground when they are spreading in the atmosphere.

Atmospheric pollution starts with the *production* of pollutants—often as undesirable or incidental consequences of various industrial processes. An airborne cycle for the pollutants begins with the *emission* of pollutants; emission is followed by *transfer* through the atmosphere, and the cycle is completed by the *contact* of pollutants with people, livestock, vegetation, or any other objects. This contact may result in the elimination of the pollutants from the atmosphere, or it may be followed by the repetition of a similar cycle. The final stage of atmospheric pollution is its possible *damage* to health and property.

Control

Production, emission, transfer, contact, and damage—this is the story of atmospheric pollution. At

each of these stages the reduction of the danger of contamination can be attempted, and in most cases scientific and technological methods for this purpose already exist (*1*). Those methods consist essentially of one or more of the following: (i) improvement of chemical or physical processes with consequent reduction of the quantity of pollutants produced; (ii) collection of pollutants at their source; (iii) transformation of noxious pollutants into inoffensive ones; and (iv) improvement of the methods for evacuating pollutants into the atmosphere. When these methods are sufficient to eliminate the inconveniences and dangers of contamination and are still economically acceptable, then the atmospheric pollution problem can be easily solved. Often, however, under certain meteorological conditions, there still remains too much pollution even after all these methods have been applied.

The question then arises: What other methods can be used to reduce further the atmospheric contamination? An obvious answer is that one should improve the methods already existing and, more particularly, develop new ones that will give better results. Science and technology will have to play an important role in such development. As in many other technical problems, scientific work on atmospheric pollution depends on basic research in various fields of science. That a better understanding of those problems is necessary and that basic research will be helpful should be self-evident. But what is important to know now is: What can we do about atmospheric pollution with the help of the knowledge presently available? Can we, for instance, eliminate the emission of pollutants into the atmosphere from the various industrial plants or other sources of pollution without altering the useful operation of the industry or of those sources? The answer is a very simple one: Yes, we can.

You may then wonder why it is not being done in those urban areas where atmospheric pollution is an already well-recognized nuisance. There are no doubt many reasons for the lack of effective action, but they do not include the one that says action is technically impossible because of our limited understanding of the basic problems of atmospheric pollution. If we are ready to pay the price of the necessary equipment and to accept the possibly complicated methods of pollution abatement, we can reduce the contamination to any desired degree. The expenses and inconveniences required to achieve these results should be measured by the importance that is placed on the need for the reduction of atmospheric pollution.

How important is the reduction of atmospheric pollution at present? There is still need for much research to provide a quantitative determination of the dangers, inconveniences, and costs of atmospheric contamination. Nevertheless, it is a recognized fact that several urban areas produce too much pollution already and that a further increase in the contamination of their atmosphere may become very dangerous under certain meteorological conditions. Urban areas must therefore face the problem of reducing the degree of contamination without at the same time seriously affecting many of those human activities that are responsible for the contamination and which, as much as the clean air, are necessary to the population.

Zoning of an urban area for the purpose of eliminating or limiting atmospheric pollution has been considered in several instances. The usual purpose of zoning in urban planning is the regulation of residential building. However, the principles and methods used in such zoning cannot be applied directly to atmospheric pollution zoning. In the regulation of residential building, one is concerned with zoning the *use of the land*; in the regulation of atmospheric pollution, one should be concerned with zoning the *use of the atmosphere* above the land. In the present article (*2*), I shall refer to such *air zoning* and I shall examine how it can be achieved.

An urban area usually includes residential and industrial buildings. Both the industrial and residential parts of the area contain sources of pollution. In a residential area, those sources may include house chimneys, motor car exhausts, household incinerators, and others. In the industrial area, we may have smoke stacks, oil refineries, steel mills, and other sources of industrial pollution. We should differentiate between two general areas of contact of pollutants with the ground. The first is the area near the source of pollution itself, and the second is the area at a considerable distance from the source. Most of the industrial sources emit very large amounts of pollutants but often emit the pollutants through tall stacks and avoid the high concentrations of pollution at ground level. A large part of the industrial pollutants comes therefore in contact with the ground at considerable distances from the industry that produces the pollutants. In residential buildings, automobiles, and house incinerators, the most important area of contact of pollutants with the ground is near the source of pollution itself.

The problem of air zoning is to limit the amount of atmospheric pollution tolerable at various locations in the area. In a populated community such limitation may be based on health requirements, nuisance level, visibility, or the pollution deposits on buildings, cars, and other objects. In rural areas

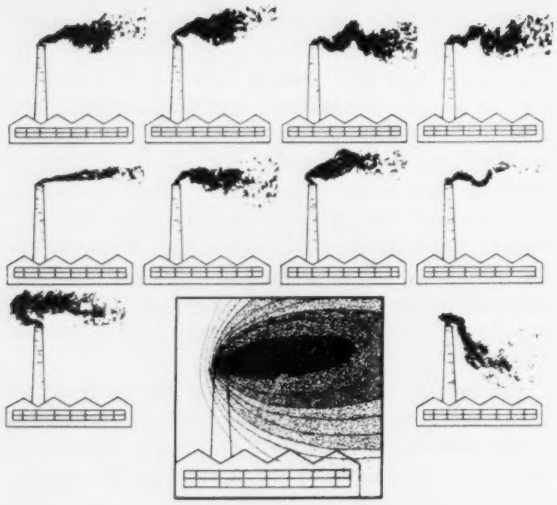


Fig. 1. Schematic illustration of various types of smoke plumes that can occur under the same meteorological conditions. Mean concentration distribution for an average smoke plume in the framed figure can be compared with theoretical results.

one may require that the atmosphere not be contaminated to the degree at which it poisons the soil, damages crops and vegetation, or injures livestock.

Atmospheric Dispersion of Pollutants

The most obvious dangers of contamination are limited in some areas to the periods of time when the meteorological conditions are very unfavorable to the dispersion of the pollutants. Since not much can be done about the weather, consideration is often given to shutting down sources of pollution when such unfavorable conditions prevail. If such extreme measures should be necessary, it would be essential to reduce them to a minimum. It may indeed often be sufficient to reduce only partially the operation of the sources of pollution and to apply this measure only to some of the sources. It will also be important to predict the unfavorable atmospheric conditions in advance to avoid the application of such measures after the damage has started. What then is the most important information about the atmospheric conditions required for such a prediction? It appears to concern primarily the following factors: the velocity of the wind that carries the pollutants, the characteristics of the turbulence that disperses them, and the nature of the temperature inversions that confine them to the lower levels of the atmosphere. With the exception of precipitation, these are the most important atmospheric factors with which we should be directly concerned.

One of the most common illustrations of turbulent diffusion, which is directly related to atmospheric-pollution problems, is the dispersion of smoke emitted from a stack. The dispersion of the smoke plume is caused by two principal factors: (i) the general air motion that carries the smoke downstream and (ii) the turbulent velocity fluctuations that disperse it in all directions. Figure 1 illustrates some shapes of smoke plumes under the same atmospheric conditions. The nature of atmospheric turbulence makes impossible a correct prediction of any of the individual examples of smoke plumes. One can, however, describe mathematically an average smoke plume and relate the mean concentrations of smoke at each point of this plume to the statistical characteristics of turbulence and to the mean wind velocity. The equations of turbulent diffusion for a continuous point source, which may be used to represent the emission from a smokestack, can be written explicitly under some simplifying assumptions, particularly when the mean wind velocity remains constant and the mass and size of the dispersing particles can be neglected (*3*).

The theoretical equations give the mean concentration distribution of dispersing particles as a function of the statistical characteristics of turbu-

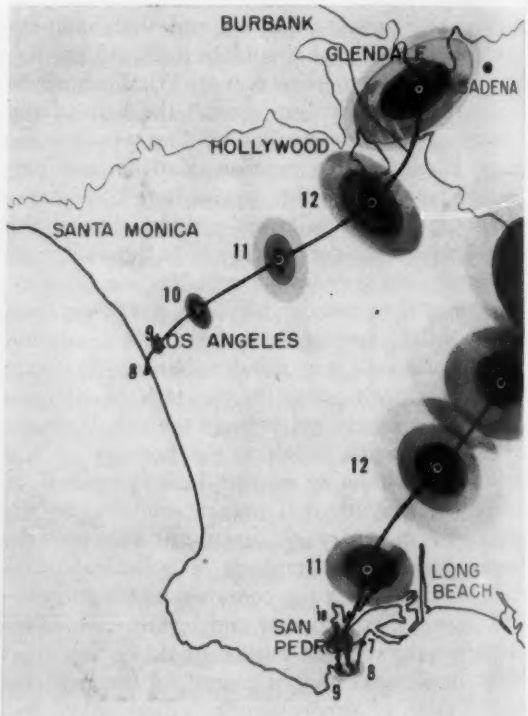


Fig. 2. Hypothetical dispersion of smoke puffs released at two points of the Los Angeles basin at 7 A.M. and 8 A.M., respectively.

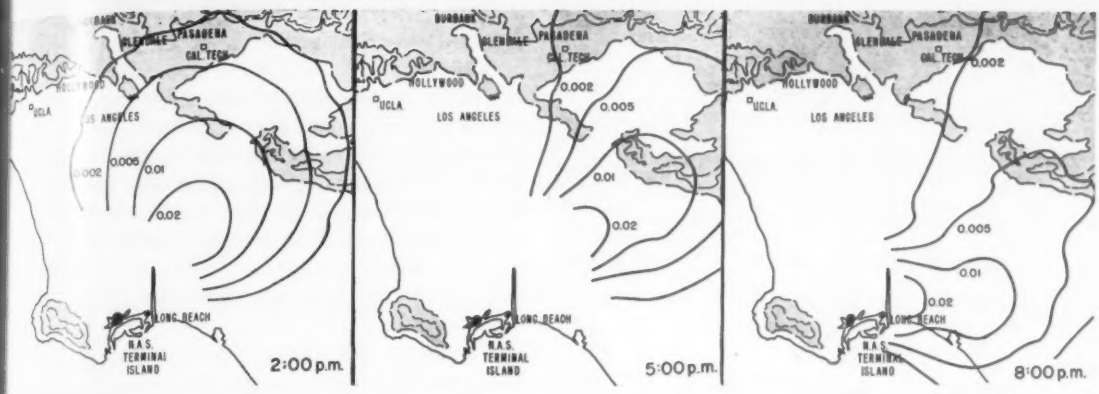


Fig. 3. Isoconcentration curves at 2 P.M., 5 P.M., and 8 P.M. for a single point source continuously emitting particles at a constant rate located at Long Beach-Wilmington (effects of topographic features and inversion not included).

lence and of the mean wind velocity. These equations can be used to determine isoconcentration curves similar to those represented in Figure 1 in the framed illustration. Each of these curves is a locus of points at which the mean concentration is the same. These theoretical curves should be compared with the mean concentrations measured using a large number of individual smoke plumes (similar to the ten plumes illustrated in the figure) and observed under the same general meteorological conditions.

Mathematical Model

A mathematical model of the atmosphere over an urban area can be used to study the probable pollution patterns. One of the simplest models can be constructed by including in the description of the model the distribution of pollution sources, their emission conditions, and the micrometeorological characteristics that directly affect the dispersion of pollutants. The mean concentration distribution of pollutants due to each source of pollution can be determined, and the effects of the several sources can be added. One can then find the mean concentration pattern of pollution over the urban areas as a function of the time. The relative contributions of each of the sources of pollution to the contamination at various points of the area can then be analyzed. Under some meteorological conditions, there exists a thermal inversion above the ground that confirms the dispersion of pollutants to the lower levels of the atmosphere. In our mathematical model such an inversion will be represented by proper boundary conditions under which the thermal inversion and the ground reflect the dispersing particles.

In the examples described earlier we have assumed that there exists a well-defined mean wind velocity that is approximately constant in magnitude and in direction for the whole urban area. However, in some cases it may not be possible to define such a constant mean wind velocity. Figure 2 illustrates how one may be able to construct a mathematical model of atmospheric pollution when the "mean" wind velocity cannot be considered as being the same for the whole area. On this figure an outline of Los Angeles County is illustrated with two trajectories determined from measured hourly wind streamlines. Let us consider, for instance, the point marked with the number 7. We assume that a puff of smoke has been emitted above this location at 7 A.M. The puff is carried downstream with the general wind velocity along the trajectory. At the same time, the turbulent wind fluctuations disperse the smoke puff to sizes illustrated by the elliptical figures for successive hours following the emission. A smoke puff emitted at the same point but at another time may follow a different trajectory and have a different rate of dispersion. By adding the effects of a large number of similar smoke puffs emitted at the same point at successive times, one can find the mean concentration pattern produced by a continuous point source.

Such a study has been made for Los Angeles County. The main difficulty in the analysis was the lack of appropriate micrometeorological measurements for the Los Angeles basin. Although some very valuable data on hourly flow patterns are available (*4*), data on turbulent fluctuations are not sufficient. It was therefore necessary to make several assumptions concerning the characteristics of turbulence. Figure 3 represents some results of this analysis. The point source is located in the Long Beach–Wilmington area; it is assumed that

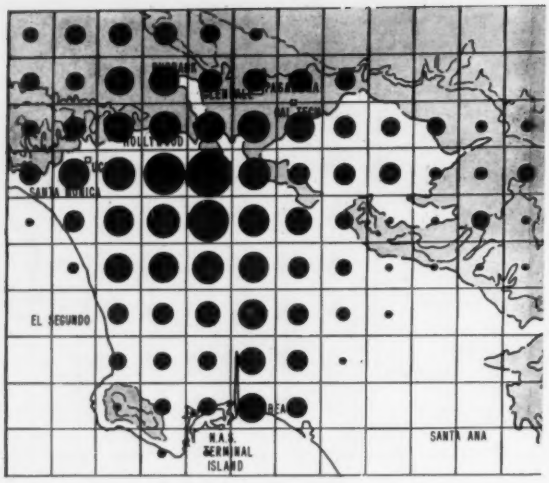


Fig. 4. Geographical distribution of traffic in the Los Angeles area. Area of circles is proportional to the number of vehicles; each square represents 16 square miles.

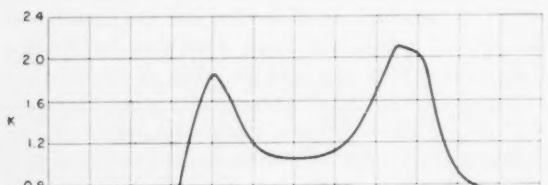


Fig. 5. Example of hourly variation of traffic volume in Los Angeles.

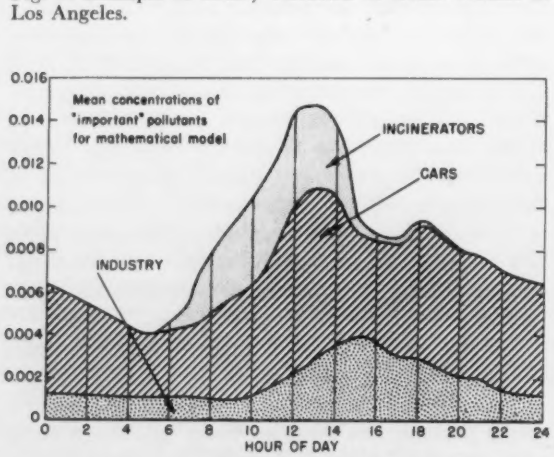


Fig. 6. Relative contributions of the three principal pollution sources to the mean concentration at California Institute of Technology. The effects of topographic features and inversion are taken into account in the mathematical model of Los Angeles County. The relative proportion of the "important" pollutants (including acids, organics, and nitrogen dioxide) emitted by the three principal sources are based on recent studies in Los Angeles County.

it emits pollutants at a constant rate. The mean concentration distributions at 2, 5, and 8 P.M. have been determined, and the corresponding isoconcentration curves are traced on the figure. The values referring to each of the curves indicate the relative magnitudes of the mean concentration. In this analysis we have not taken into account the topographic situation of Los Angeles and have assumed that there is no temperature inversion.

Relative Responsibilities of Pollution Sources

A more complete study has been made to determine the theoretical mean concentration at one location in Los Angeles County—California Institute of Technology in Pasadena. We have determined at this location the mean concentration due to two point sources, one in the Long Beach–Wilmington area and another in El Segundo, as a function of the hour of the day. Moreover, we have assumed a thermal inversion at 1500 feet above sea level and have found the probable mean concentrations using the average meteorological conditions in Los Angeles County as measured in September 1947. We have further taken into account the topographic situation of the Los Angeles basin by including in our model the San Gabriel mountains, which are north of Pasadena.

Sources of pollution distributed over a large area such as motor vehicles, gasoline service stations, home incinerators, and house chimneys can be represented as an area source. An area source can be considered as a large number of point sources spread over the area or represented by an appropriate mathematical equation corresponding sufficiently closely to the real area source. In order to illustrate the analysis for an area source, we shall consider the motor-vehicle traffic distribution in the Los Angeles basin. The geographic traffic distribution is based on some of the available data (*5*), and the density of traffic in squares of size 4 by 4 miles has been determined. In Fig. 4, the area of each black circle placed at the center of the square is proportional to the corresponding traffic density. Figure 5 represents the hourly variation of the traffic volume measured at a particular location in Los Angeles County. Obviously, the hourly traffic distribution does not change in each of the squares in the same way; however, we shall assume that it does for our model, because more complete data on the traffic variations were not obtainable. A model of the geographic traffic distribution has been described by a mathematical equation, and the pollution from the resulting area has been studied, taking into account hourly changes in the traffic volume.

Using similar methods, it is possible to include in our mathematical model of Los Angeles County several other sources of pollution and to consider their relative contributions to the mean concentration at each locality in the area. In what follows, I shall try to illustrate what kind of data can be found on these relative contributions of various sources of pollution. We shall first add to our mathematical model an area source representing the pollution from refuse incinerators. Not having available information on their geographic distribution in the area, we have used the same distribution as we did for the motor-car traffic, which is approximately comparable to the distribution of population. We have further assumed that the incinerators are operated between 6 and 10 A.M. with an hourly variation presenting a maximum at 8 A.M. Finally, we have considered the case when the relative amounts of pollutants emitted in our mathematical model during a period of 24 hours by the three sources—industry, motor cars, and incinerators—are 29, 55, and 16 percent, respectively. These percentages were derived from data referring to the emission of "important" pollutants as evaluated for 1954 (*6*). Figure 6 represents the relative contributions of each of the three sources to the mean concentration at California Institute of Technology as functions of the hour of the day. It should be emphasized that these results cannot be directly compared with measured mean concentrations at the institute. In this example we have not, indeed, taken into account the chemical reactions of the various pollutants that may occur before they reach the point of measurement. We may, however, mention that some of these reactions are related to a photochemical action on hydrocarbons that in the presence of nitrogen oxides produces oxidants (*7*). An example of the results that may be obtained, taking into account such chemical reactions, is shown in Fig. 7.

Let us now consider a residential neighborhood with its own sources of pollution that may include automobiles, indoor heating, home incinerators, and so forth. Figure 8 illustrates a small neighborhood with two houses and one incinerator. We should differentiate between the contamination from the over-all sources of urban pollution and the contamination from the local incinerator. The over-all sources of urban pollution include, for instance, the numerous industrial sources represented in our mathematical model. If our residential neighborhood is located near California Institute of Technology, then the mean concentration originating from the over-all sources of urban pollution is represented by curves similar to those in Figs. 6 and 7. For the present discussion, let us use the

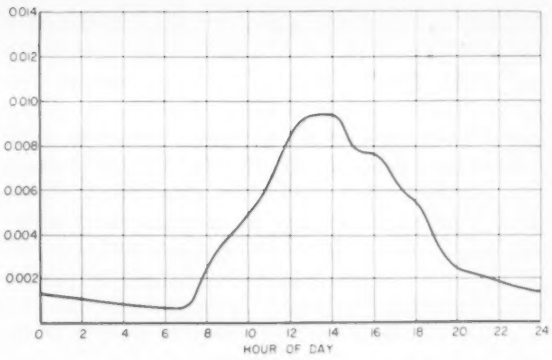


Fig. 7. Hourly variation of theoretical mean concentration at California Institute of Technology for the mathematical model of Los Angeles County, including topography and inversion. Based on recent data on the photochemical production of oxidants by pollutants, and calculated by including the effects of the photochemical reaction during daytime.

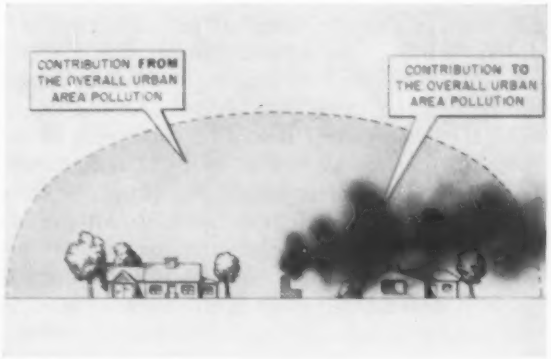


Fig. 8. Illustration of a residential area with the pollution from its own sources and from the over-all urban pollution sources.

curves in Fig. 6 and consider what should be the expected mean concentration of pollutants, say, at 8 A.M. From the scale used in the figure, we find a mean concentration of 0.0078 at 8 A.M. While the relative amounts of pollutants emitted in the urban area by industry, motor vehicles, and incinerators are, respectively of 29, 55, and 16 percent, the contributions of these three sources to the mean concentration at the institute at 8 A.M. are 13, 51, and 36 percent, respectively.

The operation of the local incinerator will of course contribute to the over-all pollution. However, before they lose their identity by mixing with pollutants from many other surrounding sources, the pollutants originating from the local incinerator may largely increase the pollution in the neighborhood. A similar problem to the one concerning a local incinerator may occur in industrial or com-

mercial areas. For instance, at a busy crossroad a large number of cars may have to stop periodically for traffic signals; during their deceleration and idling periods they emit large quantities of pollutants. Each such crossroad may in itself be considered as a local source of pollution that contributes largely to the contamination of the neighborhood. There will be many other similar cases where one may have to differentiate between the *over-all sources of urban pollution* and the *local sources of neighborhood pollution*. In the study of air zoning, the effects of both classes of sources must be taken into account.

Allowable Pollutant Concentrations

The purpose of air zoning is to limit the allowed maxima of pollutant concentrations in the atmosphere of an urban or rural area. These maxima are determined by the individual and combined characteristics of the principal pollutants and the degree of inconvenience and damage that is to be tolerated. The effects of long exposure to low concentrations and the effects of immediate exposure to high concentrations should be considered in relation to damage to health, crops, and livestock, as well as to the deterioration of property. It is necessary, therefore, to take into account the allowable long-exposure concentrations and the allowable threshold concentrations beyond which harmful and damaging effects will result. A quantitative determination of the safe limits of concentration is still an object of research studies.

In determining allowable threshold concentrations, one must take into account several factors involving not only the receiving end but also the emitting origins of atmospheric pollution. Too stringent limitations of allowable levels of pollution may impose great costs and difficulties in operating many indispensable or useful activities of the population. On the other hand, insufficient restrictions may result in harmful effects on the health of the population or damage to property. Therefore, the costs and operational difficulties of limitations on the allowable levels of pollution should be measured by the beneficial effects of such reductions.

Temporary Emergency Measures

One may wish to consider some temporary emergency measures that would limit or modify the operation of the sources of pollution and reduce the contamination. One could, for instance, consider changing the hours of operation of the incinerators to shift their contributions to pollution to other hours. The contribution of motor cars can be modified by changing, for the emergency period, the traffic pattern or traffic regulations. As a result, idling and deceleration periods, during which the motor cars emit large amounts of pollutants, would be reduced and the over-all time of operation of motor cars would be shortened. The industrial pollutants may be reduced, for the emergency period, by changing the hours of operation of some of the industrial sources of pollution. The emergency measures would, for instance, attempt to distribute the pollution concentration more uniformly over the 24-hour period to keep it below a chosen value for the allowable threshold concentration at any hour. They must of course be applied in time to be effective; this requires a quantitative prediction of the mean concentrations to be expected under various meteorological conditions.

The emergency measures modifying the operation of the various pollution sources may not always be sufficient and the stoppage of some of these sources may have to be considered. The effects of the modification in the operation or of the complete stoppage of the sources of pollution can be analyzed not only as far as the three general sources of pollution (industry, motor cars, and incinerators) are concerned, but also with reference to individual sources or groups of sources. In our present example, we have considered the concentration of pollutants referred to in Fig. 6. It may be necessary to examine the pollution levels for other pollutants as well. A similar analysis would, of course, have to be made for other locations in the urban area or in the surrounding rural areas. A complete analysis will therefore require finding the patterns of pollution over Los Angeles County, similar to those illustrated in Fig. 3. If the air-zoning requirements take into account the differences between the nature of possible damages in each of the locations, then the allowable threshold concentrations may vary with the location.

This analysis refers to the mathematical model in which the mean concentration patterns due to the over-all urban pollution sources are considered. To approach more closely the real conditions, one must take into account the possible departures from the mean concentrations and the added effects of local sources of neighborhood pollution. The mean concentration patterns were determined for the average meteorological conditions for the month of September. In our mathematical model, the description of the meteorological conditions was based on hourly mean streamlines as measured in Los Angeles County in September 1947. On each day of this month the streamlines depart from this average. This departure results in a departure of the mean concentration patterns,

for a particular day, from the results obtained from the analysis. However an important part of the present analysis is to try to forecast the mean concentration patterns to determine in advance whether any emergency measures are required. An analysis based on general meteorological conditions corresponding to a period of the year, such as the month of September, may be sufficient for such a forecast. In the final result one would, however, take into account the characteristics of the temperature inversion for the particular day for which the mean concentrations are determined since they have a major influence on the levels of concentration.

Reduction of Pollution from Existing Sources

To avoid using temporary emergency measures too often, one may study the possibility of permanently correcting an unsatisfactory pollution situation. One would first examine the effect of the local sources of neighborhood pollution in each locality. In the case illustrated in Fig. 8, one would evaluate the contribution of the incinerator operation to the pollution level. From this evaluation it can be determined whether the contribution of the local sources of pollution to the neighborhood pollution is too large; where necessary, remedies can be applied. This may correct many local situations where the main responsibility for the pollution lies with the local sources.

The concentration of pollutant for a section of an urban area represents the added contributions of its local sources and the over-all urban sources. Air-zoning principles may be based on a limitation of the proportions by which the local sources and the over-all urban sources may contribute to the allowable concentrations of pollutants. These limitations do not refer directly to the amount of pollutants emitted into the atmosphere, but to the mean concentrations of pollutant. To avoid contamination near the ground in an industrial zone, tall stacks are often used. While such stacks do reduce the atmospheric pollution of the local neighborhood, they change little, if at all, their contribution to the over-all urban pollution, particularly if there is a low-level temperature inversion.

For pollutants formed by a photochemical reaction, such as certain oxidants, one may wish to determine whether a nighttime dispersion will reduce the contamination sufficiently to justify the expense of special equipment or the inconveniences of different working hours. One could also consider the possibility of collecting the pollutants during unfavorable meteorological conditions and dispersing them into the atmosphere at a later time. In this connection, the use of natural underground caves and caverns can be considered whenever they can be found near industrial sources of pollution.

The problem of reducing the over-all pollution from such area sources as motor-cars, incinerators, house heating, and so forth, requires very serious study. In the case of incinerators, consideration is often given to replacing them by other methods of refuse disposal. The possible reduction of pollutants can be evaluated by taking into account the contamination for which an alternative method may be directly or indirectly responsible. As far as motor-car pollution is concerned, several abatement methods are considered. They include the use of devices that would modify the exhaust gases into inoffensive pollutants, the use of special fuel, and modification of engines. Serious consideration should be given to a general analysis of traffic patterns. An extensive use of expressways and roads without traffic lights would greatly reduce the contribution of motor cars to over-all urban pollution. In many urban areas, one may wish to examine how an increase in public transportation, including building subways, would affect the pollution pattern. An analysis of the driving habits of the population and the conditions of the car engines could determine whether proper improvements would reduce the fuel consumption. In an urban area with hundreds of thousands of automobiles, a small improvement for each car should not be neglected. The effects of such undesirable habits as excessively long warm-up periods, rapid decelerations, and parking with idling engines could be evaluated.

Air-Zoning for New Pollution Sources

All these studies are closely connected with air-zoning problems for an existing urban area. Their purpose is to reduce the pollution of both the over-all urban area and the local neighborhood. Such a reduction may be needed not only when there is danger of reaching the allowable threshold concentrations at any location of the urban area but also to make possible the installation of new industries or other sources of pollution that accompany a normal expansion of a city. A study of the influence of a new source of pollution on the mean concentration patterns may therefore be an important air-zoning problem. It should be emphasized that, as far as air zoning is concerned, there is no need to impose restrictions on the installation of an industry at any location of an urban area. Limitations should refer only to the amount of pollution that such a new industrial source of pollution would contribute to the atmospheric pollution of its neighborhood and of the over-all urban area.

Conventional land zoning has as its main purpose promoting art and amenity in urban building. The urban area is divided into residential, commercial, industrial, and other zones with specifications describing the character and the use of the authorized buildings and enterprises. To accomplish its purposes in each zone, the means used by land zoning are to apply certain restrictions to the buildings in the same zone. Air zoning proposes to limit the contamination of the urban atmosphere. The air zoning would specify the allowable levels of concentration for the atmosphere of each zone. However, building restrictions for each zone must take into account the contributions of the whole urban area to the contamination of its atmosphere. The necessary restrictions will therefore depend on the meteorological conditions of the urban area and result in different requirements for the same classes of zones, according to their location in the area. When building restrictions are considered, one must of course determine some specifications that would apply for all weather conditions. Such restrictions could be based on an analysis of the meteorological conditions for various periods of the year, using data for several years. One must keep in mind, however, that there will always be a certain probability of exceptional meteorological conditions that would produce an abnormally high contamination. The probability of such an occurrence can be reduced to any desirable degree by appropriate zoning regulations. It should be noted that it is not necessarily advisable to base the building restrictions on the most unfavorable meteorological conditions. One could, for example, employ temporary emergency measures when such conditions prevail and in consequence be able to relax the restrictions.

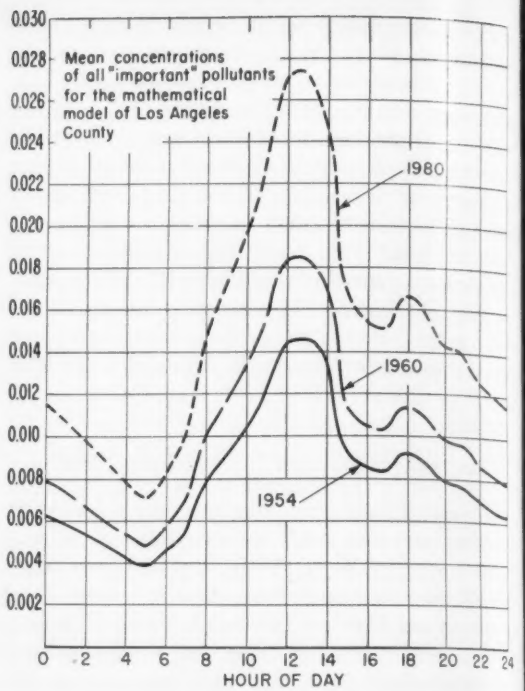


Fig. 9. Estimates of hourly variation of mean concentration at California Institute of Technology for 1960 and 1980, for the mathematical model, compared with the 1954 values taken from Fig. 6. Based on estimates of population and industrial growth and assuming no major change in their geographic distribution in Los Angeles County.

Urban Planning

A growing urban area must expect an increase of atmospheric pollution unless a greater effort in its abatement measures balances the effects of additional sources of pollution. As an example, we shall use again Los Angeles County and forecast the mean concentration at California Institute of Technology for 1960 and 1980. This forecast will be based on data concerning the future increase in population, fuel consumption, and so forth. To simplify this computation, we shall use the same mathematical model of Los Angeles County as we have used before. The result illustrated in Fig. 9 is determined for the mean concentration of the most "important" pollutants. In this figure, the curve for 1954 is the same as the one already shown on Fig. 6; the results for 1960 and 1980 assume that the abatement methods have the same efficiency as those for 1954. If we assume that the allowable threshold concentration is, say, 0.010, then the mean concentration for 1960 will be larger than this allowable threshold value between 8 A.M. and 2:30 P.M. and in 1980 between 7:45 A.M. and 1:45 A.M., as against the period 9:45 A.M. to 2:30 P.M. for 1954. In our mathematical model, these results correspond to the same meteorological conditions for each of the three cases. Obviously, the probable number of days when the meteorological conditions will be favorable for high concentrations of pollution in 1960 and 1980 will be larger than in 1954. If the frequency and the severity of above-allowable concentration periods should increase in the future city, then it must be carefully considered by the city planners. Indeed, in that case, the pollution situation is one of the parameters that must be taken into account when projecting the population gains into the future.

The problem has some similarity to the well-known interrelationship between rabbits and grass. If a large amount of grass is available, the rabbits, having sufficient food, reproduce rapidly. But, with

their increased number, they consume more and more grass and finally lack food. This in turn results in a lower reproduction rate and a drop in the number of rabbits. Following that more grass grows, more rabbits appear. As a result of this process, both the amount of grass and the number of rabbits may fluctuate over several years. In atmospheric pollution, it is quite possible to have a similar problem, although it will be affected by more than two factors.

Let us, for instance, consider the development of an urban area surrounded by rural areas and let us assume that the area has at present enough pollution to cause concern. If for some reason this urban area attracts new inhabitants and new industries, then this will result in an increase in atmospheric pollution. Following this increase, the area may become less attractive to new inhabitants and the rate of growth expected by the city planners may drop. In addition, the rural areas may become so contaminated that a larger quantity of crops and livestock is destroyed. Much of the necessary food, the quantity of which increases with the population, will then have to be brought from greater distances. This may or may not result in an increased transportation problem, which would be followed by added pollutants. It will, in any case, increase the living costs, which again will affect the attractiveness of the urban area to new inhabitants. On the other hand, some discouraged farmers may decide to give up their farms to an industrial or residential expansion that, together with the possible growth of surrounding cities, will increase the pollution of the urban area.

To these various effects, one must also add those that will have an influence on the population that still remains in the expanding urban area. The increased health hazard and the higher probability of lethal concentrations of pollution may have disastrous consequences. Such disasters may not necessarily result in a reduction of the population by death, but they may cause some of the inhabitants to leave the urban area. Although we have not included all the parameters that have an influence on the population growth, it should be obvious that this process is more complicated than the one concerning rabbits and the grass. It may, however, be followed in a similar way by a fluctuation of both the number of inhabitants and the level of contamination over a number of years. This discussion may or may not apply to Los Angeles County, but it is presented at this time to indicate the need for including in city planning an air-zoning and atmospheric-pollution control study. Although one should expect that the future will bring many practical solutions to atmospheric pollution control, such expectations cannot be included in a serious planning of urban development.

References and Notes

1. Many of these methods are reported in the *Proceedings of the United States Technical Conference on Air Pollution*, L. C. McCabe, Ed. (McGraw-Hill, New York, 1952) as well as in the general technical literature.
2. This article is based on a paper presented at the annual meeting of the Air Pollution Control Association in Detroit, Mich., 22–26 May 1955. The studies described were supported by the Bureau of Ordnance, Department of the Navy, under contract NOrd 7386. I am indebted to O. J. Deters for his help in many phases of the preparation of this manuscript. I wish to express my appreciation to Betty Grisamore for her assistance in the numerical computations and to Doris Rubenfeld and J. W. Brubaker for the preparation of the figures.
3. F. N. Frenkiel, *Advances in Appl. Mechanics* **3**, 61 (1953). See also the references given in this paper.
4. H. F. Poppendiek *et al.*, *A Report on an Atmospheric Pollution Investigation in the Los Angeles Basin* (Univ. of California Departments of Engineering and Meteorology, Los Angeles, 1948).
5. G. P. Larson, J. C. Chipman, E. K. Kauper, "A study of the distribution and effects of the automotive gases in Los Angeles," presented at the meeting of the Society of Automotive Engineers, Detroit, 10 Jan. 1955. (*S.A.E. Quart. Trans.*, in press.)
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Schools need not preach political doctrine to defend democracy. If they shape men capable of critical thought and trained in social attitudes, that is all that is necesstry.
—ALBERT EINSTEIN.

Radar Echoes from Birds and Insects

LEWIS L. BONHAM and LAMONT V. BLAKE

Mr. Bonham received his training at Purdue University and the University of Maryland. From 1943 to 1954 he was associated with the Naval Research Laboratory in Washington, D.C., and he is now a senior project engineer at the Corvey Engineering Company in Alexandria, Va. Mr. Blake, who was associated with the Arkansas Power and Light Company from 1937 to 1940, is a search-radar consultant at the Naval Research Laboratory. He received his training at the University of Massachusetts and the University of Maryland and has done part-time teaching at the University of Maryland.

IF a radar engineer were asked, "Can a radar detect a bird in flight?" he would answer, possibly after the slightest hesitation, "Yes." He would give this answer without necessarily ever having seen an echo from a bird on a radar screen or having heard reports of such echoes. For it is a known fact that any material body will reflect radar waves. Of course, the best reflectors are metallic objects, and the larger these are, the stronger the reflected signal. Since birds are neither metallic nor very large, their potentialities as radar targets seem to have been for the most part ignored. Yet it has been becoming evident that birds at sea are sufficiently effective as targets to clutter up a radar screen at close and moderate ranges when they are present in sufficient numbers. The odd thing about this phenomenon is that it has apparently gone unrecognized by many who have seen it.

Since we undertook preparation of this article, we have discovered that there have been previous studies leading to recognition of birds and insects as sources of radar echoes. However, the results of some of these studies do not seem to have been published except in reports of limited circulation.

Undoubtedly the earliest observation of a radar echo from a bird was made by R. M. Page of the Naval Research Laboratory in February 1939; it was made with an experimental 200-megacycle-per-second radar, the XAF, on the *U.S.S. New York* in Atlantic waters around Puerto Rico. In a report of this first radar operation on a naval vessel, Page wrote (*1*) ". . . an echo was picked up at a range of 9000 yards when nothing could be seen visually in this direction. . . . The range decreased to 5000 yards and then stayed at that point for some time. After a careful search with 10-power binoculars, a boatswain's bird was discovered in the (radar) beam. . . . Then his range began to increase. . . . He was followed to 11,000 yards before the signal disappeared. That the echo was coming from this bird is certain because (in addition to correlation of radar and visually observed positions) the signal was sometimes observed to fade violently and the fading was correlated with the flapping of the bird's wings."

The earliest extensive study of echoes from birds that has come to our attention was made by a British organization called the Army Operational Research Group, which published a report by D. Lack in 1945 (*2*). The report discussed work dating back to 1941. Presumably because of inclusion of data on military matters, this excellent report was classified "restricted" and is not available for general use.

We have been engaged in radar research and development for the U.S. Navy. Occasionally this work has taken us to sea for a short cruise aboard a Navy vessel. On such a trip, in 1950, one of us (Bonham) had called to his attention a "clutter" of unexplained echoes on the plan-position indicator (PPI) of one of the search radars. The ship was some 100 miles or so off the Maryland-Virginia coast; the month was March.

The radar operators referred to these echoes as "phantoms" since they did not seem to correspond to any real targets. They were a source of some annoyance. Yet they were not a serious problem because they occurred to a maximum range of perhaps 20 to 25 miles, with the majority of them inside the 10- to 15-mile range. The targets of principal interest on an air-search radar are well beyond these ranges. This probably explains why no serious attempts had been made to find the cause of these "phantom" echoes.

Bonham made extensive observations of the behavior of these targets during two brief cruises in March and April 1950 and continued the study during another short cruise, on another ship, in June of that year. He concluded that the echoes were unquestionably from birds. In January 1952, while on a similar trip, Blake noted the same phenomenon, and brought back some photographs of the radar indicators.

When the radar operators were asked directly what they thought was the cause of these echoes, they gave various answers such as "weather," "sea echoes," or simply "I don't know." When the suggestion was advanced that these were echoes from birds, they tended to reject the hypothesis. They probably felt, understandably, that birds are not large enough to give appreciable reflection of radar waves, and also they probably felt that the density of these targets seen on the radar screen was far greater than the density of birds seen in the air visually. Actually, neither of these objections is valid.

Other hypotheses have been advanced to explain these echoes, and these will be discussed; but in our opinion, the evidence overwhelmingly favors the theory that they are echoes from birds in flight.

The density of these bird echoes varies from day to day and even during a day. Sometimes they form a dense mass of echoes at close ranges—say out to 10 miles. On other days they are thinned out enough to allow operators to distinguish the separate echoes. They are seen when the sea is rough and also when it is very calm, and at all times of night as well as during the day.

The high density of bird echoes sometimes seen on the radar screen is deceptive. It creates the illusion that if one were to go out on deck and look over the rail he should see swarms of birds filling the sky, whereas actually only a few birds can usually be seen. But it must be remembered that the radar display collapses a relatively large volume of space onto perhaps 100 square inches of cathode-ray-tube screen, and the ratio of the size of the luminous echo to the total screen area is much greater than the ratio of the actual size of the bird to the corresponding area of the sea. (This is because of the pulse duration of the radar and the width characteristics of the antenna beam.) If, for example, there were on the average only one bird for every square mile of sea area, the number of bird echoes expected inside the 10-mile range ring on the radar PPI would be more than 300—enough to cause an appreciable clutter of echoes.

When the density of bird echoes is not very great, one of them can be singled out and tracked —that is, a plot can be made of its position as a function of time. It was characteristic of the echoes that they retained their identity for appreciable lengths of time (minutes), fluctuating and occasionally fading, as do echoes from most moving targets of complex shape. When a particular echo was tracked, it would usually be lost after some minutes. Sometimes the echo would just disappear, suggesting that the bird may have "landed" on the water or have flown into a null of the radar's sea-reflection interference pattern (*3*). At other times, the track would be lost in a mass of echoes from other birds.

Between the hours of 11 P.M. and 6 A.M. on the night of 15–16 June 1950, Bonham, assisted by the ship's radar observers, obtained plots of the flight paths for a number of these birds; after correcting for the ship's motion, he computed the ground speeds. He found them to be distributed over a range from about 10 to 50 knots. The flight paths were also distributed with respect to direction, but with a decided preference for a northerly course. The surface wind at the time was about 16 miles per hour in a westerly direction, and the ship's heading was southwest. The preference for a northward direction, coupled with the time of year (spring), suggests that many of the birds were migrating northward.

The average air speed of about 14 of the targets that were tracked was 27 knots, and the highest air speed was 55 knots. The individual targets were tracked for times ranging from 5 to 25 minutes. Thus the relatively minor position inaccuracy of the radar data does not cause appreciable error in the computed speeds and directions.

On one of the days when tracks of these targets were made, the wind velocity was taken for altitudes up to 6000 feet as well as at the surface. The wind speed was fairly uniform up to this altitude, averaging about 16 miles per hour, but the direction underwent a rotation of 50° at 1000 feet and 70° at 6000 feet with respect to the direction of the surface wind. It was found that the targets tracked on this day did tend to align themselves with the winds at upper levels, which seemed to lend support to a hypothesis that the phantom targets are wind-borne. But it happened that the agreement of the "preferred direction" of the tracks with the direction of north was even better than the agreement with upper-level wind direction; in addition, some of the tracks were in directions significantly different from the wind velocities recorded at any altitude. This is not absolutely conclusive, for the radar will detect targets at altitudes higher than 6000 feet. But the most significant factor tending to disprove the hypothesis that the echoes are from windborne targets is the discrepancy in speeds. The maximum wind velocity up to 6000 feet was about 20 miles per hour, which is appreciably lower than even the average speed of the targets that were tracked.

As may be guessed from this discussion, the suggestion has been put forward that these phantom echoes are some sort of atmospheric phenomenon

Fig. 1. Plan-position indicator (PPI) of an air-search radar that operates at a wavelength of about 25 centimeters. The faint concentric rings are 5-mile range markers; the figures in outer ring are true-bearing angles. The discrete luminous spots are bird echoes. The diffuse luminous area in the lower left quadrant is a weather front. The bright solid patch in the center is partly sea return and partly bird echoes.

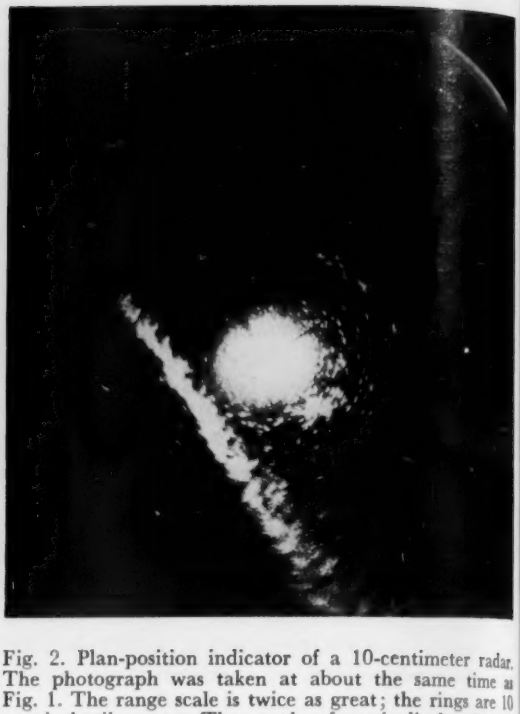

Fig. 2. Plan-position indicator of a 10-centimeter radar. The photograph was taken at about the same time as Fig. 1. The range scale is twice as great; the rings are 10 nautical miles apart. The weather front is displayed more prominently at this wavelength and sea return extends to almost 10 miles, obscuring many bird echoes that can be seen at 25-centimeters, but some are seen at ranges between 10 and 20 miles in the east-to-south sector.

that would, of course, go with the wind. It is well known that some types of atmospheric effects will cause radar echoes; but the phenomena of these kinds that are known do not give a discrete, well-defined echo. For example, weather "fronts" of the kinds illustrated in two of the accompanying photographs are diffuse and extensive. (The echo from a weather front probably results almost entirely, if not wholly, from raindrops.)

The proposal that reflections from atmospheric discontinuities—small "regions" differing in dielectric constant from the surrounding atmosphere—would cause radar echoes resulted from mysterious echoes first reported by a Bell Telephone Laboratories engineer, M. W. Baldwin, who was working at the Chesapeake Bay Annex of the Naval Research Laboratory in 1944 (*4*). They were observed on a microwave fire-control radar that was being developed for the Navy by Bell Laboratories. The echoes, dubbed "Angels," could not be correlated with any visible physical objects. Similar observations were made at the Bell Telephone Laboratories at Holmdel, N.J., (*5*) and at the Signal Corps Engineering Laboratories at Belmar, N.J., (*6, 7*), and the theory was advanced by Baldwin and others that the echoes were the result of "reflecting regions in the atmosphere." At least one scientific paper has been presented in support of that theory (*8*). However, A. B. Crawford of Bell Telephone Laboratories, working in cooperation with the Navy Electronics Laboratory at a field site in Arizona, reported some experiments in 1949 that tend to refute this hypothesis and to establish insects and birds as "the chief, and possibly the only, sources of these reflections" (*9*). While some disagreement still exists on this matter, the experiments reported by Crawford are quite convincing. An excellent discussion of the matter has been presented by D. E. Kerr *et al.* (*10*).

Another well-known source of echoes from natural phenomena is the "sea return," which is, as the name implies, made up of echoes from the surface of a rough sea. Here again, however, there is no similarity between sea return and the "phantom" echoes. Sea return forms an approximately circular area surrounding a ship of essentially "solid" fluctuating echoes with an irregular but fairly well-defined edge. It is not possible to select a discrete echo and track it. The extent of the sea-return area depends on the roughness of the

sea, the height of the antenna, and the radar characteristics—particularly power, beam width, pulse length, and frequency. The "phantom" echoes are definitely not sea return. On one occasion when they were seen in greatest quantity, the sea was exceptionally calm.

The radars on which echoes from birds have been observed by us range in frequency from about 1000 to 10,000 megacycles per second, and it may be significant that in most cases they are the more sensitive radars designed for long-range detection. There is no reason to believe that bird echoes would not be observed on radars of any frequency commonly used, for large birds have some dimension that is an appreciable fraction of a wavelength even at a frequency of 200 megacycles per second. In fact, as mentioned in a previous paragraph, R. M. Page of the Naval Research Laboratory saw echoes from a bird on the experimental 200-megacycle XAF radar in February 1939 (*1*). This was of course a relatively low-powered radar by present-day standards, yet it had a large antenna and a high pulse rate, and it used "searchlighting" and A-scope display instead of the less sensitive but more useful azimuth scanning and PPI display.

The "phantom" echoes reported here are seen well enough to be noticeable only on the PPI displays of sets of more recent vintage that use relatively high power and sensitive receivers. This is of course part of the answer to the objection that birds are too small to give echoes of significant strength. Any material object will reflect a significant amount of energy if the transmitted power is great enough and if the receiver is sufficiently sensitive.

A bird does have an advantage as a radar target—its height. As is well known, it is difficult to see very low-lying radar targets on the sea because the interference between direct and sea-reflected waves produces virtually zero field intensity at the sea surface (*3*). But the signal strength increases rapidly with height. For this reason, a bird flying at altitudes well above the sea would be equivalent in radar detectability to a considerably larger target on the sea surface. Moreover, since radar echo signal power for elevated targets varies inversely as the fourth power of the distance, a bird at 10-mile range would return as strong an echo signal as an airplane target approximately 600 times as large (in equivalent reflecting area or "radar cross section") at 50-mile range, if both were at the same elevation angle.

Nevertheless, birds are small targets, and the echoes received from them at these short ranges have the general characteristics that echoes from more conventional targets (airplanes) have at much greater ranges—that is, they fade or fluctuate and occasionally disappear. At these short ranges—20 miles or less—echoes from airplanes are very strong on the long-range search radar, practically saturating the receiver amplifiers and "blooming" on the cathode-ray-tube screen. This is the behavior that radar operators have come to expect at these ranges from even the smallest targets of naval interest on air-search radars, which probably explains their disregard of these relatively weak, fluctuating echoes from birds.

Following the observations in 1950, inquiry was made of John Aldrich, an ornithologist of the U.S. Fish and Wildlife Service, concerning the characteristics of sea birds to see if they would agree with the radar observations. Additional comments were obtained from Richard Tousey, a physicist of the Naval Research Laboratory who is also a student of ornithology. It turned out that little information concerning flight speeds is available. It may well be that radar information on this subject will be superior to that obtainable by any other method. However, Aldrich thought that the speeds recorded were reasonable. The bird he considered most likely to be present at considerable distance from land is the shearwater. Gulls do not ordinarily go more than 60 miles from land. Many of the observations of the "phantom" echoes were made at night, and the shearwater is a partially nocturnal bird. In appearance it is somewhat like a gull, but it flies at great distances from land. Moreover, it is a migratory bird, and the peak of the northward migration period is about April. However, Tousey pointed out that the shearwater flies close to the water, gliding in the air motion produced by wave crests, and rarely reaching altitudes above 10 feet. Thus it would be a poor radar target at ranges beyond 1 or 2 miles. To be easily detectable beyond 10 miles, a bird should fly at altitudes of at least 100 feet. In addition, the shearwater would not ordinarily be in the air during periods of very calm sea, and possibly would not be found as far south as Maryland and Virginia.

The gannet was suggested as another possibility. This is a rather large bird, white with black wing tips, that would be present in this area from about January through April, but not in June. Still other possibilities during April are migratory land birds, which fly fairly high (for example, 1000 feet) and fly at night as well as during the day.

On one of the cruises mentioned, a variation in number of bird echoes was noted as the ship approached land. The number increased noticeably when land came within sight, and the directions of flights also became more random, suggesting that the birds being seen were now gulls or other

Fig. 3. View over rail of ship at sea in January 1952. One bird, probably a herring gull, of the type that causes radar echoes is visible. Distance of bird from camera is estimated to have been about 100 feet. Most of the birds seen on radar are too far away to be visible in the photograph.

birds of the coastal waters that were not migrating.

Actually, a radar of any power level is theoretically capable of detecting a bird—if the bird is close enough. But on microwave radars the sea return masks echoes from birds at very close ranges except under very calm sea conditions. (The intensity and range of sea return increases with frequency.) This fact made it impracticable to attempt visual correlation with the radar observations made at sea on radars operating at frequencies above 1000 megacycles per second. Recently, however, an attempt was made to obtain such correlation at the Chesapeake Bay Annex of the Naval Research Laboratory at the suggestion of Tousey, who thought it should be possible to see birds several miles away with high-power binoculars. But the binoculars were unnecessary. Because of the relatively calm water in the bay, it was possible to see bird echoes at considerably less than a mile on a relatively low-powered 10,000-megacycle-per-second "navigational" radar. The birds could be observed visually at the same time, and the correlation of positions and movements was excellent, leaving no doubt that the birds were the cause of the observed echoes.

In case anyone who reads this feels amazed that radar should be able to detect targets as small as birds, an experience of one of us (Blake) should be recounted in which targets of a still smaller order of magnitude—namely, insects—were believed to have been seen as discrete echoes. This observation tends to confirm the previously mentioned conclusions of Crawford of the Bell Telephone Laboratories (*9*). In July 1951 some experiments were being conducted with shore-based radar located on a high cliff overlooking the water at the NRL Chesapeake Bay Annex. One of the radars was a now obsolete 3-centimeter gun-laying radar that had an exceptionally short pulse (considerably less than 1 microsecond), very narrow beam (less than 1 degree), and very rapid scanning of a limited azimuth sector (10 degrees). The signals were presented on a type B cathode-ray-tube indicator. The echoes on a display of this type appear as luminous spots, as on the PPI, but the raster is rectangular instead of circular, with azimuth angle and range as abscissas and ordinates, respectively, of the Cartesian coordinate system. Only a few miles of range were presented and the resolving power of the system was very high.

It was noted that the first couple of hundred yards or so of the display was cluttered with a great many discrete echoes that moved about quite rapidly in a random fashion (the type B scan magnifies the apparent rapidity of angular motion at close ranges in the same way that the Mercator projection maps magnify distances near the earth's poles). The question arose, what caused these "phantom" echoes? There certainly were no visible moving targets of conventional nature in the area. Yet it seemed that there must be something actually there, reflecting the radar waves, and moving about.

The thought occurred that they could conceivably be insects. From the edge of the cliff, looking out in the direction of the radar beam, insects could be seen. It was a warm summer day, and there were insects of all descriptions flying about. Some of them were easily large enough to be a reasonable radar target at 3-centimeter wavelength —that is, the dimensions of a good-sized June bug or dragon fly would certainly be at least one-half wavelength. The motions of the echoes seen on the screen corresponded to reasonable speeds of insects in flight, and the density of echoes was similarly reasonable, when the considerations already discussed for relative densities of birds and bird echoes were taken into account.

Finally, the question how so small a target could reflect enough to give a detectable signal is again answered by reference to the very short range, of the order of only 200 yards or so. An insect at 200 yards will return the same echo signal power as a target approximately 10,000 times as large (in "radar cross-section") at a range of 1 nautical mile. Thus the primary requirements for seeing insects on a radar screen are the use of short

wavelengths and sufficiently short pulses to permit seeing at very close range. The narrow beam width, rapid scanning, and type B indicator were advantageous for revealing the motion of the insects; it was really this that called attention to them. If they had appeared stationary, they would have been thought to be echoes from nearby fixed objects due to side lobes of the antenna pattern.

Less study was given to this phenomenon than to the bird echoes, partly because circumstances did not permit it. Yet it is believed that there is really no question that the echoes were actually from insects.

Over a period of years, many reports of "phantom" targets on radar have been received from naval vessels and other radar installations. The characteristics of some of the reported echoes may possibly require explanations other than those suggested here. But it is believed that birds are responsible for a large class of such reports of "phantoms," and insects may account for some others. The observations of birds may be of interest to ornithologists. By means of radar they may be able to gather data on flight speeds and densities in space that would not be practically obtainable in any other way. A. B. Crawford has made a similar suggestion concerning possible use of radar by entomologists for observing the density of flying insects (*9*).

References and Notes

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2. D. Lack, British Army Operational Research Group Report No. 257 (1945), restricted classification.
3. This is due to the familiar Lloyd's mirror effect of optics, which produces an interference pattern of alternate lobes and nulls in a vertical plane above the reflecting surface, with spacing dependent on the height of the source above the surface and on the wavelength of the radiation. Ideally, this occurs only when the sea is smooth, but actually the effect is observed even when the sea is fairly rough if the angle between the sea surface and the incident ray is small, and it is an important effect in radar detection of targets over water.
4. M. W. Baldwin, "Radar echoes from the nearby atmosphere," paper presented at the joint meeting of International Scientific Radio Union and Institute of Radio Engineers in Washington, D.C., 2 May 1946.
5. H. T. Friis, *Proc. Inst. Radio Engrs.* **35**, 494 (1947).
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7. M. W. Baldwin, *ibid.* **36**, 363 (1948).
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9. A. B. Crawford, *ibid.* **37**, 404 (1949).
10. D. E. Kerr *et al.*, *M.I.T. Radiation Laboratories Series.* Vol. 13, *Propagation of Short Radio Waves* (McGraw-Hill, New York, 1951), pp. 591–595.

Above all I wish to oppose the now widespread and seemingly plausible opinion, that a question in physics is only worth investigation if from the outset the fact that it admits of a definite answer is established. If the physicists had always followed this precept, the celebrated experiment of Michelson and Morley on the measurement of the so-called absolute velocity of the earth would never have been made. . . . So our efforts to ascertain the absolute velocity of the earth have proved exceedingly fruitful for science, although nowadays the question itself is almost universally considered to be meaningless. —MAX PLANCK, in "The concept of causality," *Proc. Phys. Soc.* 1 Sept. 1932.

LETTERS

Betwixt and Between Dates

The day is a fundamental natural unit of measure and it is important in any chronological sequence. Our everyday calendar is an artificial medium that, while adequate for most social purposes, is cumbersome to use when an exact interval of days is required. Many different tables, slide rules, and other media have been devised to satisfy individual needs. All of these devices are designed to eliminate the use of weeks, months, or other combinations of days.

There are many fields of endeavor where the number of days between dates, days elapsed, days remaining, and the date of the end or the beginning of any given number of days are everyday requirements. Yet, one of the most useful tables of all is little used; and, for that matter, there is ignorance of its existence among many whom it would benefit most. This could be understood if it were not readily available—but it is; and it includes all dates in the past and future that one might want to use. It is a table that is widely used in astronomy and is commonly referred to as the Julian Day calendar, or Julian Day number. With it, the interval between two dates for which the Julian Day number is known can be found by subtracting the smaller number from the larger. It is as simple as that! But what is this Julian Day number that makes it so easy to determine betwixt and between dates? Where does it come from? Where does it begin?

There is little need to recount the trials and tribulations man has had in trying to make a calendar that would be compatible with various natural phenomena. Although our present Gregorian calendar is less than 300 years old, it is not very satisfactory, and there is a concerted effort today to amend it once again. In addition, we are familiar with the trouble Caesar had with the calendar, and we know that the year 1582 is referred to as the "year of confusion." But the year 1582 also can be referred to as the year of light, calendarwise, for Joseph Scaliger came up with a bright idea. He suggested that the common multiple of three well-known cycles—Solar, Metonic, and Roman indiction—would give a period of 7980 years which he proposed as a universal measure of chronology. He called it the Julian Period.

The solar cycle of 28 years refers to the recurrence of the same day of the week on the same day of the year. It was introduced about A.D. 325, and the cycle was fixed by establishing the year A.D. 1 as the tenth year of the cycle.

The Metonic cycle of 19 years is named after the Greek astronomer Meton who observed that 19 solar years, approximately, are equal to 235 lunar months. This cycle refers to the recurrence of the new moon on the same days of the year. It was introduced about A.D. 530, and the epoch of the cycle is fixed by the new moon that occurs on 1 January.

The Roman indiction cycle of 15 years has no astronomical basis and refers to the 15-year periodic valuation of property for assessment—in other words, taxes. It was established about A.D. 321, and the epoch of the cycle was fixed by calling the year A. D. 1 the fourth year of the cycle.

Scaliger showed that when the 28-year solar, 19-year Metonic, and 15-year Roman indiction cycles were carried backward they had a common beginning in the year 4713 B.C. (Julian calendar). Therefore, he fixed 1 January 4713 B.C. as the beginning of his comprehensive Julian Period, the end of which will occur in A.D. 3267.

Using 1 January 4713 B.C. as a base, Scaliger also established his Julian Day number, whereby each day is given a number representing its numerical succession since the beginning of the Julian Period. The first day

Table 1. Portion of 100-year "work card" showing the Julian Day Number for the 0 day of each month, from 1951 to 2000 (*3*). Examples: 1 January 1956 = J.D. - - - 5474; 23 June 1956 = J.D. - - - 5648.

Year.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1956	35473	35504	35533	35564	35594	35625	35655	35686	35717	35747	35778	35808
1957	35839	35870	35898	35929	35959	35990	36020	36051	36082	36112	36143	36173
1958	36204	36235	36263	36294	36324	36355	36385	36416	36447	36477	36508	36538
1959	36569	36600	36628	36659	36689	36720	36750	36781	36812	36842	36873	36903
1960	36934	36965	36994	37025	37055	37086	37116	37147	37178	37208	37239	37269
1961	37300	37331	37359	37390	37420	37451	37481	37512	37543	37573	37604	37634
1962	37665	37696	37724	37755	37785	37816	37846	37877	37908	37938	37969	37999
1963	38030	38061	38089	38120	38150	38181	38211	38242	38273	38303	38334	38364
1964	38395	38426	38455	38486	38516	38547	38577	38608	38639	38669	38700	38730
1965	38761	38792	38820	38851	38881	38912	38942	38973	39004	39034	39065	39095

of January 1956 was Julian Day 2,435,474. The Julian Day number provides a simple tool that enables us to extract the number of days between dates easily and with little effort. In addition, the Julian Day number is readily correlated with the days and months of the Gregorian calendar.

The Julian Day number is published in several different forms. The annual calendar of the American Association of Variable Star Observers (*1*) has been known for 40 years as the AAVSO Julian Day Calendar (*2*), and it has an international distribution. A "work card" having limited distribution (*3*) is a table giving the Julian Day number for the 0 day of each month from A.D. 1900 to 2000. The annual *American Ephemeris and Nautical Almanac* (*4*) includes a summary table from which the Julian Day number can be obtained for any day from 1 January 4713 B.C. to A.D. 2019. These three forms are all that anyone needs to find quickly and easily the number of days betwixt and between dates.

The most useful form, of course, is the 100-year "work card," unless short periods within a calendar year are involved. No comparison of the Julian Day method with other methods in use is necessary. However, an example of its use will be sufficient to show its simplicity.

Suppose one wants to find the number of days from 15 October 1956 to 20 March 1959. Although the Julian Day number has seven figures, it is common practice to use only the last four. Using the 100-year table, a portion of which is shown in Table 1, one obtains the following:

20 March 1959 = J.D.	6648
15 October 1956 = J.D.	5762
Number of days =	886

If 886 days is a period, and it is desired to know when the previous period began, the process is reversed:

15 October 1958 = J.D.	6492
Period =	886
Beginning of period = J.D.	5606
May 0, 1956 = J.D.	5594

Period began 12 May 1956

The use of the Julian Day number is not confined to special fields of work. Even the general medical practitioner as well as others who deal with periods of gestation can make good use of it. Gestation periods given in months, weeks, and years may be good enough for the general public, but for science they are given in days. It is easy to see why—because the number of days varies in the month and the year. Here is a comparison of two published gestation tables, scientific and popular: (i) chicken—21 days and 3 weeks; (ii) dog—62 days and 2 months; (iii) human being—280 days and 9 months.

The importance of the periods in days is clearly brought out here, for 9 months could be 276 days (May through January), or it could be 273 days (January through September). A 2-month period may vary from 59 to 62 days.

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References

1. American Association of Variable Star Observers, 4 Brattle St., Cambridge 38, Mass.
2. United Scientific Co., 204 Milk St., Boston 9, Mass. About 15,000 AAVSO Julian Day Calendars for 1955 were distributed.
3. Planning and Research Associates, 45 Bromfield St., Boston 8, Mass.; 100-year work card.
4. *American Ephemeris and Nautical Almanac* (Superintendent of Documents, Washington 25, D.C.), annual.

Form and Symmetry in Organisms

The essay by Paul Weiss, "Beauty and the beast: life and the rule of order" [*Sci. Monthly* **81**, 286 (1955)], invites me irresistibly to a letter of comment. My errand is to communicate a classroom practice that I have pursued for years. It bears directly on Weiss' ideas on the form and symmetry and structure of organisms.

In the standard course in analytic geometry, we study an array of familiar loci (the straight line, power functions, conics) and a number of curves (algebraic and transcendental) characterized by special geometric properties. These last possess intriguing *esthetic* form and symmetry, and I call attention to the fact that forms in nature subscribe to these mathematical (geometric) properties, *often identically*. After we have plotted such curves as the spiral of Archimedes, the logarithmic spiral, the hyperbolic spiral, the cubical parabela, the semicubical parabola, the cissoid of Diocles, the lituus, the cardioid, the various-leaved roses, and so on (there are literally scores of special curves), I suggest that students find leaves of trees and petals of flowers that "fit" these curves. It is astonishing to discover the precise identity between a curve plotted on coordinate paper and a leaf or petal superimposed upon it.

Illustrations are endless: a certain pine grows two needlelike leaves (pine needles) in a cluster that fits the semicubical parabola ($x^3 = ay^2$); some fit the cissoid of Diocles [$y^2 = x^3/(2a - x)$]; the twining tendril of beans and other clinging vines fits certain spirals, such as Euler's spiral or the lituus ($r^2\theta = a^2$); leaves and flower petals in abundance fit the various "roses."

Indeed, as one thinks about it, is there *anything* in nature that does not possess *mathematical* form?

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Comments on Crop Yield Data

In Roy W. Simonson's very interesting article [*Sci. Monthly* **81**, 173 (1955)], his picture of increased yields of farm crops is painted in colors that are, perhaps, a little too bright. He compares figures for 1948 with those for the 1935–39 period. In making such a comparison, it is always dangerous to use figures for a single year. Weather has such a marked effect on yields that large yearly fluctuations inevitably occur.

The year 1948 happened to be one of favorable weather, but from 1932 to 1938 our farmers were afflicted with such a succession of bad years that many of them were driven to despair and some to bankruptcy. The 1930's, therefore, offer a poor basis for comparison. To obtain a clear idea of the changes that have occurred, it is advisable to go back 50 years and compare the figures for successive 10-year periods. For five leading grain crops, we then obtain the statistics shown in Table 1.

Table 1. Average yields in bushels per acre.

Years	Wheat	Rye	Corn	Oats	Barley
1901–10	14.5	13.3	28.3	28.9	24.0
1911–20	14.1	12.7	26.2	31.0	22.8
1921–30	14.2	12.5	25.3	29.7	22.8
1931–40	13.4	11.3	24.0	27.8	20.6
1941–50	17.2	11.1	34.7	33.0	24.9

The percentage increase of the last period over the first is 18.6 for wheat, 22.6 for corn, 14.2 for oats, 3.8 for barley; rye shows a decrease in yield of 16.5 percent. The increase in the case of corn is mainly due to the introduction of hybrid corn and has been obtained only at the cost of producing corn that is higher in carbohydrate and lower in protein.

As the result of 50 years of scientific effort, these figures are not impressive. When we compare our all-time high for wheat of 19.5 bushels per acre, with Canada's 26.5, the United Kingdom's 45, or Denmark's 47, it is difficult to find much cause for satisfaction.

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I can appreciate Leonard Wickenden's objection to comparisons of agricultural production in 1948 with that of the previous 5-year period. The single year was chosen as representing a longer interval; the comparison would have been the same had the 5-year period of 1947–51 been used. This 5-year period was actually used in reporting increases of yields of potatoes, corn, cotton, and wheat [*Sci. Monthly* **81**, 180 (1955)]. Moreover, the moving averages given by B. T. Shaw [*Agron. J.* **45**, 85 (1953)] show marked trends toward increased yields during the past 15 years for potatoes, corn, cotton, wheat, eggs, pork, and dairy products. Similar trends are evident in the data given in *Agriculture's Capacity to Produce* [*U. S. Dept. Agr. Inform. Bull. 88 (1952)*].

Increases paralleling those in the United States have also been obtained elsewhere. A note on the FAO decennial [*Science* **122**, 866 (1955)] includes this statement: "FAO, in reviewing agricultural development of the past decade, says that world agricultural production was more than 25 percent greater in 1954 than it was in 1946–47 (excluding the Communist bloc). The world is now producing about 20 percent more rice, milk, and cotton than it was before the war; about 30 percent more wheat, fats, and meat; 50 percent more fruit and sugar; and 80 percent more natural rubber."

It is true that drouth years occurred during the 1930's. The 5-year period of 1935–39 did not include the drouths of the early 1930's, although it did include the one dry year 1936. Production of wheat, corn, and livestock in some sections of the country was lower in 1936. The impact of the drouth years on total agricultural production for the country as a whole was not substantial, which is also indicated by the average data for grain yields cited by Wickenden. The trend toward higher yields seems clearly evident over and above any effects of drouth.

My contention is the recent trends toward higher production reflect a growing importance of science and technology to agriculture. Examples in addition to those cited in my paper are plentiful. For example, the average yield of corn in Virginia in the last 3 years has closely approached that of Iowa, which demonstrates the importance of fertilizers, improved varieties, and other findings of science and products of industry. The increased yields of potatoes in recent years and the higher productivity of pastures in the southern states are additional reflections of the same factors. Numerous other examples can be taken from recent experiences in this country. Consequently, I still think it clear that there has been a recent increase in production, which is ". . . by no means the realization of the full potential within science and technology." Achieving the potential agricultural production within the limits of the present scientific and technologic resources of this country would, however, entail many problems.

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Science seldom renders men amiable; women never.—BEAUCHENE.

BOOK REVIEWS

Plant Taxonomy. Earl L. Core. Prentice-Hall, New York, 1955. xiv + 459 pp. Illus. $7.50.

The appearance in 1951 of Lawrence's excellent *Taxonomy of Vascular Plants* threatens to let loose a flood of pent-up manuscripts on the same subject. Since there has been little agreement on how taxonomic botany should be taught, it is hoped that subsequent authors will present a wide variety of approaches to the subject. Core, however, follows Lawrence all too closely, although with little more than half as many pages; his volume, also, is divided into the same two parts. Despite considerable differences in arrangement and except for a chapter on botanical exploration and a few pages on botanical gardens, virtually everything to be found in the first part of Core's book has its antecedent in Lawrence's. Core does place more stress on historical development. Indeed, he is at his best in relating vignettes of botanical history and exploration.

On the other hand, Core is much briefer in his treatment with regard to biosystematics and cytogenetics, taxonomic research techniques, and the extensive literature of the field. His point of view may be summed up in the following sentence, snatched forcibly out of context. "Fortunately, it has been discovered that genetic relationships are usually reflected in similarities and differences in morphological structures, which are readily detected by the eye, usually on brief examination." Would that this were always so!

The eight lines devoted to anatomical criteria seem a bit parsimonious, and the only reference to physiological criteria for classifying vascular plants has to do with the highly suspect data on "serum diagnosis" emanating from the Königsberg school. Under morphological criteria we are offered the ostensibly phylogenetic ones of Bessey; it is to be assumed that the author accepts these, since he lists them without qualification of any kind. In view of this fact, it is a little disconcerting to find the quite contradictory Englerian system employed in Part II, again without qualification. I find a little distressing the "either/or" character of some of the choices presented to the reader in the chapter on the evolution and migration of floras. Disjunct distributions are to be explained in terms of either land bridges or movable continents ("becoming more generally accepted"), and endemics are either very young species or very ancient ones.

The second part of both books, devoted to a survey of selected groups of vascular plants, is essentially similar, but Lawrence offers 492 pages with 260 figures to Core's 247 pages with 34 figures. Even this comparison is not wholly equable, for the typography of the former permits nearly 100 more words per page. Core helps to make up for this inferiority in illustration by providing a key to the families of each order. Surely everyone knows that statements made in such surveys about numbers of genera per family, species per genus, distribution, and so forth, are not to be taken too literally. Where successive authors secure data from the same sources, however, constant repetition of certain numerals or phrases tends to give them the force of dogma.

As an example, both Lawrence and Core credit the genus *Azorella* (Umbelliferae) with 100 species; despite some unsolved questions regarding the validity and limits of the related genera *Bolax, Laretia,* and *Schizeilema,* and including two or three species that I think are undescribed, there may be perhaps half that number. Core does not allow in this family for the occurrence of capitate inflorescences (for example, *Eryngium*), purple or maroon petals, or nonseparating, carpophore-less, or terete fruits. Also, *Daucus* occurs widely in the Southern Hemisphere, and the existence of *Ligusticum* in either Chile or New Zealand is very doubtful. Lawrence states that the Hydrophyllaceae occur "on all continents except Australia"; they are absent also from Eurasia. In this family, Core attributes 20 species to *Nemophila,* 4 to *Eriodictyon,* and 115 to *Phacelia* whereas my own (but by no means sacrosanct) figures for these genera are 11, 9, and 150 to 200, respectively. Other readers will notice flaws in the groups familiar to them.

Despite such inaccuracies, perhaps unavoidable in simplified and sweeping résumés of this nature, we may be grateful for the vigorous new activity apparent in systematic botany. If the textbooks now appearing on the market can reshape instruction in the field to embrace the newer research techniques and accord them their proper place while retaining the best of classical taxonomy, they will be welcome indeed.

LINCOLN CONSTANCE

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Quantitative Analysis. Albert Frederick Daggett and William Buell Meldrum. Heath, Boston, 1955. viii + 470 pp. Illus. $5.

The recent revival of interest in analytic chemistry and the desire of the writers for a textbook to meet their class needs have fostered this new book. The subject matter of classical quantitative analysis has been divided into 16 chapters (338 pages) on "Principles and techniques" and 25 illustrative experiments (122 pages), with the stated intention of separating theory and fact from laboratory procedures. The first six chapters deal with treatment of data, the balance, solutions, ionization, reactions, and equilibria; the next four, with the various aspects of gravimetric analysis; and the last six, with volumetric analysis.

The coverage of the theory topics is quite adequate but in some places almost indigestibly concise and in others confusingly interspersed with too specialized or almost irrelevant examples, and with organic formulas or mathematical examples and derivations for which many quantitative students who have not yet studied

organic chemistry or physics are not ready. One may question the inclusion of 17 diagrams illustrating various parts of and kinds of balances and weighing equipment, the detailed and perhaps not certainly established formulas of chelates (p. 81), dichlorofluorescein (p. 239), phenolphthalein (p. 274) and diphenylbenzidine (p. 303), the numerous pictures of simple apparatus and operations in Chapter VII, and many of the bulky arithmetical examples and problem calculations. The laboratory procedures section gives general instructions for the usual fundamental operations and then the procedures for the experiments (none instrumental except electrolysis). In general these are well chosen and well done; there could be disagreement with the inclusion of the three closely related titration methods for silver and halide as separate analyses, and one or more of the less often used ore analyses, and the nonuse of pure solids for student standardizations of solutions.

Every page of the book bears evidence of thought and care in choice of material and in writing, but the separation of theory and procedures, the compression of the whole content into a terse 470 pages, the large number of cross references to related but widely separated topics, and the ultramodern mixed-face, scrambled typographic design and format, all conspire to produce an unattractive and hard-reading text. The instructor who uses it will find necessary much lecturing, discussion, and individual help to his students, to give them a host of operational details and the clear correlation of fact, theory, and practice that a competent analyst must have.

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The Botany of Cook's Voyages and Its Unexpected Significance in Relation to Anthropology, Biogeography and History. vol. 14, No. 5/6. Elmer Drew Merrill. Chronica Botanica, Waltham, Mass.; Stechert-Hafner, New York, 1954. 119 pp. Illus. + plates. $4.75.

This work is devoted to a consideration of the plants collected on Captain James Cook's three expeditions around the world between 1768 and 1780. As the author points out, the significance of these early collections, which were made by competent botanists or naturalists (Banks, Solander, and the two Forsters), has been largely overlooked, yet herein lies a vast body of useful information, especially as it relates to the time and means of introduction of many exotic species of plants into insular areas of the Pacific.

The general reader will find here neither a detailed narrative of Cook's voyages nor any discussion of the native floras of the regions he visited. The author frankly admits that, in this study, he is less interested in the indigenous vegetation of the Pacific than he is in the plants of pantropic distribution, especially the man-introduced cultigens and weeds. On an early page he summarizes his memoir in the following words: "The botanical evidence clearly indicates that, as of 1769, all the cultigens in the Pacific Islands, with possibly two or three exceptions, were brought in purposely or inadvertently from the west by the early people who occupied the Polynesian triangle." This is supplemented by the statement that there is no valid evidence that any of the important cultigens of the New World became universally distributed in both hemispheres in pre-Columbian or pre-Magellan times. The remainder of the work is concerned essentially with reinforcing, by a multitude of examples, these two basic assertions.

There is an interesting discussion of the relationship of early trade routes to plant introduction both across the Atlantic and the Pacific, and there is a detailed consideration of the histories of such important food plants as maize, pineapple, peanut, and guava, as well as tobacco and the common gourd. The author is an avowed antidiffusionist and devotes much space to controvert that school of thinkers.

A word of gratitude is due the publisher for having included in this memoir several excellent illustrations from early botanical works, especially those of Rumphius and Hernandez.

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Die Binnengewässer in Natur und Kultur. Einführung in die theoretische und angewandte Limnologie. A. Thienemann. (Verständliche Wissenschaft, vol. 55.) 156 pp. Illus. Springer, Berlin, 1955. DM. 7.80 ($1.90).

Near the eve of a creative career, Europe's "dean of hydrobiology" penned with ripe wisdom this semi-popular introduction to life and conditions for living in fresh water. Tidbits of knowledge from the world literature on aquatic ecology are woven into a few short chapters, which explain how aquatic organisms meet the rigors of life in water and how they adapt to light, temperature, pressure, currents, and chemical differences at depths and in seasons for lakes and streams from the arctic to the tropics.

Commonly the tropical lakes are neglected in discussions of inland waters, but Thienemann has here skillfully extracted a statistical sample of facts from his own and Ruttner's voluminous reports on their joint expedition to Java. Here, also, he shows us the importance of these waters in the economy of tropical peoples.

One seldom hears of a conservation movement in Europe, but in this booklet a lively condemnation of wasteful water practices is to be found. Europe's water economy is authoritatively analyzed. Effectively chosen examples emphasize the limited supply of fresh water for electric power, irrigation, culinary use, chemical industries, and sewage disposal. Land drainage, deforestation, and the increasing artificial changes in rivers and streams force Thienemann to warn Europeans of the ever-narrowing margin of safety as the population increases. From his chapter "Water, the concern of Europe," I cite a few examples. The Danube digs itself 1 inch deeper each year between Ulm and Passau—result: a drying up of large areas along the river owing to

a loss of ground water to the lowering river channel. Another river bed, the Wertach, is now 30 to 56 feet deeper since regulation. Ground water now runs down its banks into the canal like river. Shortages of ground water are in most instances traceable to man's activities, not nature's. He quotes Demoll's sermon on the water engineer's biological commandments. "1. Thou shalt not increase the flow of water from the land at a greater rate than normal. 2. Thou shalt attempt to maintain the flow of brook and river levels as evenly as possible."

European pollution problems are also reviewed. One example must suffice. The clear waters of subalpine lake Zurich once attracted bathers and vacationers from all over Europe. Its whitefish delighted the gourmets among them. Fertilizers from domestic sewage have now induced thick blooms of a reddish-colored alga that lend a sickly hue. Moreover the decomposing algae consume the oxygen of the deep waters, thus exterminating the delectable whitefish.

Condensation is primary in a popular review, but the sections on biological production and lake classification could have been reduced to allow a chapter on fishery science. There has been an unfortunate separation of limnology and fishery science in the past. I regret its continuation in a book that proposes to review the knowledge of inland waters.

This booklet is volume 55 of a series on *Understanding Science* (*Verständliche Wissenschaft*) edited by Karl von Frisch. One is inclined to generalize about the heavy, voluminous nature of German books. Here, however, is a series brief and lightly written. It reminds me of the Methuen series in English.

A glossary will aid the nonhydrobiologist.

ARTHUR D. HASLER
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Rome beyond the Imperial Frontiers. Mortimer Wheeler. Philosophical Library, New York, 1955. xii + 192 pp. Illus. + plates. $7.50.

That Rome possessed a great commercial empire extending far beyond her political boundaries has become evident as the fruits of archeological research have accumulated. This fascinating book correlates the scanty historical references with the relatively abundant material evidence to paint a coherent picture of the commerce of Imperial Rome.

The extensive trade with free Germany for hides, furs, and amber extended far up into Scandinavia and even into Russia. The trade with Central Africa for ivory, gold, and ostrich plumes has left visible traces barely touched as yet by the archeologist. The trade with India for spices and with Afghanistan for Chinese silks has been well demonstrated by the archeological evidence, although here again very little intensive digging has been attempted. Mortimer Wheeler's clearly written book will appeal to anyone interested in Roman history or in archeology.

MARK H. ADAMS
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Fact, Fiction, and Forecast. Nelson Goodman. Harvard Univ. Press, Cambridge, 1955. 126 pp. $3.

The title *Fact, Fiction, and Forecast* has a popular appeal. Most individuals, however, will be disappointed in the book itself, for it is essentially a technical presentation for philosophers, not for laymen and even less for scientists. The book comprises four lectures that do not form a unified or coherent whole.

The first lecture deals with "The problem of counterfactual conditionals." The reaction to the author's view of this problem has varied from an opinion that there is no problem at all to an opinion that the problem is insoluble. He would have us believe that "if we set aside all problems of dispositions, possibility, scientific law, confirmation, and the like, we virtually abandon the philosophy of science." One can hardly find fault with so broad a statement; it might be possible, however, to have a philosophy of science without necessarily approaching these problems from the author's viewpoint. In view of the fact that here he is approaching the entire matter from the standpoint of linguistics, one might well question his dogmatic assertion that "If we lack the means of interpreting counterfactual conditionals we can hardly claim to have any adequate philosophy of science." Throughout his work he evidently has in the back of his mind some kind of static framework of theoretical truth, but he does not reveal it. Regardless of whether or not there is such a metaphysical background, the working scientist would prefer to restrict the tentative meaning of words operationally and to treat theory wholly heuristically. Consequently, in a rapidly changing pattern of scientific theory, the scientist of today will not spend too much time trying to obtain a linguistically rigorous expression of ideas, which he regards as merely symbols of experiences. The author's own hope is that "Philosophy . . . has rather the function of explicating sciences—and everyday—language rather than depicting science or everyday procedure."

The author's use of many nondictionary words does not make his concepts easy to comprehend. For example, "a counterfactual" is illustrated by the following statement: "If that piece of butter had been heated to 150°F., it would have melted." He is concerned with the validity of the relationship between the antecedent and its consequent. Evidently the latter does not follow logically from the former without certain other information that he designates "factual conditionals." The problem, then, is to determine the suitable set of such true statements that must be "cotenable" with the counterfactual in order for the latter to be true. After considerable discussion, the author concludes "I do not see any way of meeting the difficulty" of circular involvement. Accordingly, he runs into the problem of the meaning of a natural law. Scientists would hardly regard the following statements as very meaningful without considerable amplifications: "A law is thus a sentence that is both lawlike and true. . . . A sentence is lawlike if its acceptance is not dependent upon the determination of any given instance." In this manner, induction is made to depend on the problem of con-

firmation. The author claims that "the problem of what statements are confirmable merely becomes the equivalent problem of what predicates are projectable from known to unknown cases."

The other three chapters in the book are later lectures that were given at the University of London dealing with the same general theme.

RAYMOND J. SEEGER

National Science Foundation

Contributions to Plant Anatomy. vol. 15, *Chronica Botanica.* Irving W. Bailey. Chronica Botanica, Waltham, Mass.; Stechert-Hafner, New York, 1954. xxvi + 262 pp. Illus. + plates. $7.50.

The book embodies selected reprints from Irving W. Bailey's bibliography of more than 100 papers published between 1909 and 1953. Such a volume is a tribute to Bailey: a tribute not frequently given living scientific investigators and writers. There is no doubt that the breadth and intensity of his work merit this attention. The selection and arrangement of papers are that of Bailey himself at the request of Barghoorn and Verdoorn of the *Chronica Botanica.*

Bailey's early investigations of the structural and physical properties of wood transcend mere observation and description. He has made frequent and far-reaching practical applications, and has taken time to reflect upon the broad significance, of his detailed work. For example, during World War I, he advised on the selection of wood for airplane manufacture. Somewhat later he studied national resources of timber and advised on future needs in timber production and wood products. In contrast to these practical applications, his extensive knowlege of wood anatomy led him to discern broad phylogenetic relationships. These in turn inspired interest in paleobotany as well as in taxonomic relationships among the living flora. After World War I, Bailey studied the histology and cytology of the cambium and its derivatives which eventually led him (in collaboration with Thomas Kerr) to studies on the plant cell wall and its chemical constituents.

During this period he collaborated also with William Morton Wheeler in investigating the peculiar relationships between ants and certain tropical plants, the mermecophytes. His more recent work and reflections on the comparative anatomy and morphology of vascular plants have established the signficance of secondary wood to phylogeny and, in particular, have shown phylogenetic relationships among families of the Ranalian complex. Much of Bailey's work has been related to, or in collaboration with, other disciplines; and he chose these relationships as the central theme in his selection and arrangement of papers in the present volume.

The papers are grouped in eight sections. In part I the relationships of plant anatomy to cytology and ontogeny are shown by some of his work on the cambium and its derivatives. Part II concerns his investigations of the cell wall which demonstrate the relationships of his work to chemistry and physics. Part III, "Phylogeny," includes his papers on vessel development and tracheary evolution. The relationship of plant anatomy to plant taxonomy is made apparent in part IV by three papers on the comparative morphology of certain Ranalian families. Part V, "Entomology," includes his work on "neotropical ant-plants." In part VI, paleobotanical relationships are shown by his paper concerning identification of Mesozoic coniferous wood. Anatomical relationships to wood technology in part VII are demonstrated by his work on the structure of pit membranes in coniferous tracheids in relation to the entrance of gases and liquids. Part VIII, "Cooperation in scientific research," consists of selections from his writings on the "Role of Research in the Development of Forestry," which resulted from his national investigation of natural forest resources.

Each of the eight parts bears an attractive title page designed by G. L. Swamy. Anatomical designs on each title page suggest the subject matter of the particular section, and in most sections they also convey a trend in phylogenetic specialization of xylem cells or tissues. In addition to these ornamental pages, interesting vignettes are found elsewhere, and about one-half of the original figures and plates have been reprinted.

This volume will make Bailey's work readily accessible to students of plant anatomy and allied interests. Many such students will want to own a copy. The complete bibliography and the interesting biography alone have intrinsic value. What a pity that such an interesting book bears the uninspiring title of *Contributions to Plant Anatomy*!

JANE PHILPOTT

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Introduction to Social Welfare. Walter A. Friedlander. Prentice-Hall, New York, 1955. xvii + 683 pp. $9.

Walter Friedlander, who teaches at the University of California in Berkeley, has written a textbook that is remarkable because of its scope. The book's 22 chapters are divided into three parts: a historical one, which deals with the development of social welfare in England and the United States; a descriptive one, which covers the present system and organization of social welfare; and a third one, which is entitled "Social welfare administration." Each chapter is supplied with a selected bibliography, and the book is indexed.

This book is primarily destined for the undergraduate who wants to round out his liberal arts education or who wants to enter one of the fields of social work and for the citizen who is interested in health and welfare. Friedlander has succeeded in incorporating materials that are seldom found in textbooks in this field—for instance, materials on international social welfare, industry and social welfare, and public housing and social welfare. These inclusions give this book definite advantages over other textbooks, because they bespeak an alertness to new or emerging social welfare needs and make for considerable breadth.

I am somewhat puzzled by the organization of the

materials. For instance, it is difficult to discern a rationale for the placing under the heading "Social welfare administration" material on community organization, which is usually considered a social work method, and on professional aspects of social work, which is more in the realm of issues and could be treated under a separate heading. While the author has wisely avoided an extensive and detailed exposition of the social work methods, he has already dealt with community organization in an earlier chapter called "Methods of social work."

Some readers might also have preferred that issues in the field of social work, such as questions of the scope of the field and problems of undergraduate and graduate educational content, might have been brought out more forcefully.

With reference to the field of social work education, it is particularly regrettable that there is no mention of the 1952 curriculum policy statement of the Council on Social Work Education, which represents a more recent development than the basic eight areas of social work education that Friedlander mentions. This policy suggests a division of the curriculum into three broad domains: the social work methods, the social services, and the human growth and behavior sequence.

In summary, although this book has considerable merit by virtue of the scope of its factual content, it would have more admirably fulfilled its purposes of serving the citizen and the future social worker if at the same time it contained more analysis.

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The Unified System Concept of Nature. Stephen Th. Bornemisza. Vantage, New York, 1955. viii + 137 pp. $3.

This book has a noble aim: to show the ubiquity and meaning of self-maintaining systems in nature. It spite of the provocative writings of several able men, chief among whom are W. B. Cannon, Norbert Wiener, and Alfred Emerson, the significance of such systems has not yet penetrated very deeply into the consciousness of most biologists. The reasons for the slowness of diffusion of this point of view are not altogether plain. Perhaps as much to blame as anything else is the fact that this heterodox approach demands that we think in terms of process, using a language that has a strongly substantival bias. Only slowly do we escape this linguistic prejudgement of nature by inventing such "things" as vectors and differential operators. Process is not idiomatic in the Indo-European languages, and our intellectual escape from the shackles of substance is being effected almost entirely through the use of the language of mathematics.

What a pity it is, then, that Bornemisza elects to do without this powerful language in his attempt to summarize the processistic approach to nature. When really difficult ideas are being expounded, an author's self-restriction to ordinary rhetoric may produce, at best, a tour de force like Laplace's *Essai philosophique sur les probabilités*, a work that is rich in meaning to those mathematically adept enough to recast its phrases into proper mathematical form and all but meaningless to the mathematically inept for whom it was supposedly written. With less verbal and rhetorical skill, the present author has brought forth a turgid collection of *ex cathedra* generalizations. Typical of these are the following:

(p. 18) "Metaphorically speaking, nature is endowed with a memory, a retentive faculty, which leads over and over again back into the past."

(p. 25) "The universe is inhabited by nothing but self-maintaining systems."

(p. 50) "Every active or self-maintaining source of fitness is alive."

(p. 63) "Organism and image are space-temporally equivalent with regard to their confines."

(p. 94) "The relation between body and soul necessarily is the same as the relation between the content of cognition and cognition itself."

These statements have, of course, been quoted out of context, but I think most readers of the book will agree that their meaning is not much greater in their original surroundings. The trouble is that the author somehow fails to make contact with his reader, fails to develop that counterpoint of ideas, examples, evidence and counterevidence that gives the reader the illusion that *he* is being talked to. The book is a soliloquy that surely has not had the benefit of healthy criticism by either colleagues or editors. Only by making this assumption can we account for the frequent appearance of Rabelaisian catalogs of analogous items, such as in the sentence that starts on line 8 of page 14 and ends, 23 semicolons and 57 commas later, on page 15. And the last three words of this promising entrant in the nonstop sentence derby are (naturally!) "and so forth."

Those who are desirous of learning the elements of self-maintaining systems theory are still best advised to read Cannon's *The Wisdom of the Body* or Wiener's *Cybernetics*.

GARRETT HARDIN

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Advanced Calculus. Angus E. Taylor. Ginn, Boston, 1955. xiii + 786 pp. Illus. $8.50.

The increasing need for rigorous analysis in mathematics and the other sciences has encouraged the publication of several textbooks in recent years designed to place more emphasis on rigor than seems advisable in the first year in the calculus, and at the same time to introduce topics either not treated at all in the first course, or treated too briefly. The textbook by A. E. Taylor has accomplished these purposes admirably, especially the introduction of rigor by a consideration of the real number system and some point set theory suf-

ficient for the need. Illustrative examples are well chosen as are those left for the student.

Chapter I, "Fundamentals of elementary calculus," is exceedingly well done and should not be hurried through too rapidly, since it carries the key to the later work. Chapter II, "The real number system," introduces the student to the Dodekind Cut idea, sequences and sets of real numbers, and nested intervals.

Limits, continuity, and the theorem of the mean are adequately treated in the first four chapters. The other chapters deal with the topics that have become more or less standard for this kind of book. Partial differentiation of explicit and implicit functions, vectors, multiple integrals, differential geometry of curves and surfaces, line and surface integrals, theory of integration, infinite series, improper integrals, functions of a complex variable, Fourier series and integrals are all presented with a high standard of excellence. It is obvious that with a limited time, say three hours a week for a year, the instructor will have to choose the material that may be left out or treated very briefly.

Some misprints have been observed, but they are generally so obvious that they should not cause trouble. It seems to me, however, that theorem II, page 92, should be reworded. It stands a chance of being misunderstood. It is concerned with the proof that if a set of real numbers has an upper bound it has a least upper bound. It might be better to state the argument differently, perhaps as follows: Put in the R class all numbers that can serve as upper bounds and in the L class all other numbers.

On the whole Taylor has done a supremely good job in writing a book of the type so greatly needed at the present time.

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Sir Joseph Banks, the Autocrat of the Philosophers, 1744–1820. H. C. Cameron. Batchworth Press, London, 1952. xx + 341 pp. Illus. 25s.

Australia missed being called "Banksia" and so perhaps his best known memorial is the Banksia rose, although this favorite was named for Lady Banks. For 43 years Banks served as president of the Royal Society, as unofficial adviser of kings and magistrates, as the *real* supporter of the Kew Gardens [even though his library and collections passed to the British Museum, (Natural History) through his librarian, Robert Brown], as a liberal patron of many natural history enterprises, as diplomat without portfolio who urged the return to France of scientific specimens taken by English buccaneers—in summation, he was "autocrat of the philosophers."

From Banks' first voyage to Labrador and Newfoundland, his serving in the South Pacific with Cook's First Voyage aboard the *Endeavour* [a new *verbatim* edition of Banks' journal, edited by J. C. Beaglehole, is to be published by the Mitchell Society], to his last trip to Iceland, Banks pioneered carefully planned and equipped natural history expeditions. Cameron says, "in the very year in which the British Government reluctantly agreed to recognize the independence of the former American colonies, a definite plan was made to found a new convict settlement in New South Wales." Following the clandestine introduction of merinos from Spain to England, Banks promoted the sheep industry in Australia.

H. C. Cameron's biography is the best that has appeared on Banks and shows a saturated familiarity with the subject. Occasionally Cameron forgets that he has nowhere given the reader a usefully complete title for certain works he mentions—for example, George Mackaness' biography of 1936 or Hermannsson's *Banks and Iceland* (1928)—but the omissions are minor, and the documentation is adequate. There is too little made of Banks' boyhood and of the critical role of his neighbor and mentor, Lord Sandwich, in his formative years.

Writing and receiving a stream of letters, which only now are being calendared by Warren R. Dawson [to be published by the British Museum (Natural History)], Banks' mail arriving at Soho Square included questions on cow pox, the best sorts of wheat, the interference of the exploration of the River Gambia by the slave trade, fuller's earth, the cultivation of hemp, flax, and potatoes in India and gifts of three emus from New South Wales, an albino tiger, and so forth. "Banks, all his life, was immersed in affairs. In the Italianate speech dear to the learned of his time he was a curioso not a virtuoso. . . . A curioso alive to the appeal of all knowledge."

Banks' circle of friends was as wide as the range of his interests, but he did not escape the lampoons of his detractors. Cameron gives us samples of these burlesques. "Defence or counter-attack against these anonymous onslaughts were out of the question," and he continued to make friends "without distinction, if they shared his interests . . . from Farmer George, King of England, to the tough sea-dog who ventured his life in his service." They all cross the pages of Cameron's well-knit story of a great catalytic force in British science and adventure, a happy amateur at heart who shared his wisdom and fortune wisely and unstintingly.

Joseph Ewan

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The Luminescence of Biological Systems. Frank H. Johnson, Ed. American Association for the Advancement of Science, Washington, D. C., 1955. xiv + 452 pp. Illus. $7; AAAS members, $6.

This is a compilation of 17 papers dealing with the properties and behavior of bioluminescence, chemiluminescence, fluorescence, phosphorescence, and other manifestations of "cold light" emitted by various organisms ranging in complexity from bacteria to fishes. Although the book deals primarily with recent advances, there is adequate review of earlier knowledge supplemented by 630 references. The book is the product of a week-long conference organized by the Photobiology

Committee of the National Research Council. Under the sponsorship of the National Science Foundation, the conference brought together a group of leading investigators for the purpose of making a critical inventory of present knowledge, interchange of ideas, and injection of new approaches into the problem.

In an introductory paper, E. Newton Harvey surveys the perspectives of the problem and presents a tabulation of all major luminous genera, showing their habitat, histology, and luminescent characteristics. From their analysis of luminous spectroscopy of the photosynthetic system, Becker and Kasha deal with the utilization of energy by chlorophyll on a molecular level. Arnold outlines recent observations and explanations of light emission (delayed) of green plants some seconds after having been illuminated. The fluorescence spectrophotometry of photosynthetic pigments is treated by C. Stacy French. The kinetics and mechanisms of various chemiluminescent reactions are considered in a paper by Wilhelmsen, Lumry, and Eyring. The spectroscopic investigations of luminescent systems of Spruit and Spruit-van der Burg emphasize that different organisms may have fundamentally different types of luminescent systems, which may be related to unique molecular structure and to fluorescence.

New advances in the biochemistry of luminescence are dealt with in papers by Tsuji, Chase, and Harvey (*Cypridina* luciferin), McElroy and Hastings (firefly luminescence), Strehler (firefly and bacterial luminescence), and Hastings and McElroy (properties of bacterial luciferase). The physical chemistry of intracellular luminescence is reviewed by Frank E. Johnson, who stresses the activating and inhibiting roles of temperature, hydrostatic pressure, and chemical agents.

In his paper on the physiological control of luminescence in animals, J. A. C. Nicol concludes that luminescence in animals must be regulated by glandular secretion, muscular contraction, chromatophore movement, intracellular photogeny, and possibly other mechanisms. Problems in the analysis and interpretation of the physiological control of luminescence of various kinds of animals are examined in a paper by John Buck. Y. Haneda presents an extensive review of observations made in Japan and the Far East on luminous organisms, including bacteria, fungi, dinoflagellates, macroplankton, annelids, mollusks, crustaceans, myriapods, insects, and fishes.

Ecological factors responsible for the prodigious growth of dinoflagellates, which may result in red water, mass mortalities, or luminescence in the sea, are considered in a paper by John H. Ryther. In the final paper, Haxo and Sweeney report on the cultivation and physiological characteristics of a luminescent dinoflagellate, *Gonyaulax polyedra*.

Skillful organization and uninhibited interchange of ideas during the conference and careful editing in collaboration with the different contributors has produced an exceptionally well-integrated volume with little duplication while retaining the personal points of view of the various investigators. Its usefulness is enhanced by an author index, an index of luminescent genera, and a general subject index.

CLAUDE E. ZOBELL
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Books Reviewed in SCIENCE

3 February

Quantum Mechanics, L. I. Schiff (McGraw-Hill). Reviewed by J. L. Powell.

Cardiovascular Renal Problems, H. Popper and D. S. Kushner, Eds. (Blakiston Div., McGraw-Hill). Reviewed by N. C. Moran.

Principles of Meteorological Analysis, W. J. Saucier (Univ. of Chicago Press). Reviewed by D. K. Todd.

Solar Energy Research, F. Daniels and J. A. Duffie, Eds. (Univ. of Wisconsin Press). Reviewed by M. Telkes.

Determination of Organic Structures by Physical Methods, E. A. Braude and F. C. Nachod, Eds. (Academic Press). Reviewed by A. H. Frye.

Magnetic Materials in the Electrical Industry, P. R. Bardell (Philosophical Library). Reviewed by R. M. Bozorth.

10 February

Static and Dynamic Electron, P. A. Sturrock (Cambridge Univ. Press, London). Reviewed by E. G. Ramberg.

Vitamins and Hormones, vol. XIII, R. S. Harris, G. F. Marrian, and K. V. Thimann, Eds. (Academic Press). Reviewed by H. A. Schneider.

Recent Progress in Hormone Research, vol. XI, G. Pincus, Ed. (Academic Press); *The Hormones*, vol. III, G. Pincus and K. V. Thimann, Eds. (Academic Press). Reviewed by R. Gaunt.

Protein Malnutrition, J. C. Waterlow, Ed. (University Press, Cambridge, Eng.). Reviewed by E. P. Ralli.

Microscopy of Ceramics and Cements, H. Insley and V. D. Fréchette (Academic Press). Reviewed by D. McConnell.

24 February

Weeds, W. C. Muenscher (Macmillan). Reviewed by L. J. King.

Schools of Psychoanalytic Thought, R. L. Munroe (Dryden). Reviewed by J. Dollard.

Surgical Procedures on the Gastrointestinal Tract of Animals in Preparation for Chronic Experiments, E. N. Speranskaya (Academy of Medical Sciences, U.S.S.R.). Reviewed by S. A. Corson.

Reports on Progress in Physics, vol. XVIII, A. C. Stickland, Ed. (The Physical Society, London). Reviewed by R. Schlegel.

Fifth Symposium (International) on Combustion (Reinhold; Chapman & Hall). Reviewed by F. J. Bordt.

New Books

Traité de Paléontologie. vol. V, *La Sortie des eaux. Naissance de la tétrapodie. L'Exubérance de la vie végétative. La Conquête de l'air.* Amphibiens, reptiles, oiseau. Masson, Paris, 1955. 1113 pp. Cloth, F. 12,800; paper, F. 12,000.

Hypnotic Suggestion. Its role in psychoneurotic and psychosomatic disorders. S. J. Van Pelt. Philosophical Library, New York, 1956. 95 pp. $2.75.

Proceedings of the First International Conference of Parapsychological Studies. Utrecht, The Netherlands, 30 July to 5 August 1953. Parapsychology Foundation, New York, 1955. 136 pp.

Eternal Egypt. Clement Robichon and Alexandre Varille. Trans. by Laetitia Gifford. Philosophical Library, New York, 1955. $6.

Evolution, Genetics, and Man. Theodosius Dobzhansky. Wiley, New York; Chapman & Hall, London, 1955. 398 pp. $5.50.

The Theory of Hydrodynamic Stability. C. C. Lin. Cambridge Univ. Press, New York, 1955. 155 pp. $4.25.

The Windward Road. Adventures of a naturalist on remote Caribbean shores. Archie Carr. Knopf, New York, 1956. 258 pp. $4.50.

The Secret of the Hittites. The discovery of an ancient empire. C. W. Ceram. Trans. by Richard and Clara Winston. Knopf, New York, American ed. 1, 1956. $5.

Annual Review of Nuclear Science. vol. 5. James G. Beckerley, Ed. Annual Reviews, Stanford, Calif., 1955. 448 pp. $7.

Man and the Winds. E. Aubert de la Rue. Trans. by Madge E. Thompson. Philosophical Library, New York, 1955. 206 pp. $6.

Faune de France. vol. 59, pt. 2, *Coléoptères Curculionides.* Adolphe Hoffmann. Lechevalier, Paris, 1954. 722 pp. F. 6000.

Die Physik der Hochpolymeren. vol. 3, *Ordnungszustände und Umwandlungserscheinungen in Festen Hochpolymeren Stoffen.* H. A. Stuart, Ed. Springer, Berlin, 1955. 693 pp.

The Common Sense of the Exact Sciences. William Kingdon Clifford. Edited and with a preface by Karl Pearson. Newly edited with an introduction by James R. Newman. Dover, New York, (unabridged and unaltered republication of the Knopf ed., 1946), 1955. 249 pp. Paper, $1.60.

Matter and Light: The New Physics. A survey of the contributions of Heisenberg, Compton, Einstein, Fermi, Bohr, Dirac, Planck, etc. Louis de Broglie. Trans. by W. H. Johnston. Dover, New York (original ed., *Matière et Lumière,* 1937; English ed. 1, Norton, New York, 1939), 1955. 300 pp. Paper, $1.60.

Dimensional Analysis. H. E. Huntley. Rinehart, New York, 1951. 158 pp. $3.50.

The Invertebrates. vol. IV, *Echinodermata.* Libbie Henrietta Hyman. McGraw-Hill, New York, 1955. 763 pp. $10.

Experimental Design. Theory and application. Walter T. Federer. Macmillan, New York, 1955. 583 pp. $11.

Celebrated American Caves. Charles E. Mohr and Howard N. Sloane. Rutgers Univ. Press, New Brunswick, N.J., 1955. 339 pp. $5.

The North American Midwest. A regional geography. John H. Garland, Ed. Wiley, New York; Chapman & Hall, London, 1955. 252 pp. $8.

Atlas of Paleogeographic Maps of North America. Charles Schuchert. Wiley, New York; Chapman & Hall, London, 1955. 177 pp. $4.75.

Chemistry and Human Health. Burnham S. Walker, Isaac Asimov, and M. Kolaya Nicholas. Blakiston Div., McGraw-Hill, New York, 1956. 445 pp. $5.75.

The Mathematical Analysis of Electrical and Optical Wave-Motion on the Basis of Maxwell's Equations. H. Bateman. Dover, New York (unabridged and unaltered republication of ed. 1, 1914), 1955. 159 pp. Paper, $1.60.

Partial Differential Equations of Mathematical Physics. Arthur Gordon Webster. Samuel J. Plimpton, Ed. Dover, New York (unabridged and unaltered republication of ed. 2, 1933), ed. 2, 1955. 440 pp. Paper, $1.98.

Bone: An Introduction to the Physiology of Skeletal Tissue. Franklin C. McLean and Marshall R. Urist. Univ. of Chicago Press, Chicago, 1955. 182 pp. $6.

Progress in Organic Chemistry. vol. 3, J. W. Cook, Ed. Academic Press, New York; Butterworths, London, 1955. 273 pp. $7.80.

Hochvakuum-Elektronenröhren. vol. 1, *Physikalische Grundlagen.* Akademische, Frankfurt, 1955 (Order from Academic Press, New York). 297 pp. $8.33.

Principles and Problems in Energetics. J. N. Brønsted. Trans. by R. P. Bell. Interscience, New York-London, 1955. vii + 119 pp. Illus. $3.50.

Hand Surgery in World War II. Sterling Bunnell, Ed. Office of the Surgeon General, Dept. of the Army, Washington, D.C., 1955 (Order from Supt. of Documents, GPO, Washington 25). 447 pp. $3.75.

Advances in Virus Research. vol. III. Kenneth M. Smith and Max A. Lauffer. Academic Press, New York, 1955. 339 pp. $8.

What Is Science? Twelve eminent scientists and philosophers explain their various fields to the layman. James R. Newman, Ed. Simon & Schuster, New York, 1955. 493 pp. $4.95.

Famous Problems and Other Monographs. *Famous Problems of Elementary Geometry,* F. Klein; *From Determinant to Tensor,* W. F. Sheppard; *Introduction to Combinatory Analysis,* P. A. MacMahon; and *Three Lectures on Fermat's Last Theorem,* L. J. Mordell. Chelsea, New York, 1955. 322 pp. $3.25.

Resonance in Organic Chemistry. George Willard Wheland. Wiley, New York; Chapman & Hall, London, 1955. 846 pp. $15.

The Flood and Noah's Ark. Studies in Biblical Achaeology No. 1. André Parrot. Trans. by Edwin Hudson. Philosophical Library, New York, 1955. 76 pp. $2.75.

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